

The Deadly Irony of Terrorism

THE death of Dr Aharon Katchalsky in the wild shooting at Tel Aviv airport last week is not merely an irreparable loss to the scientific community but yet another demonstration that blind terrorism does more to harm than help the causes for which it is supposedly and misguidedly intended. Katchalsky was a versatile and imaginative scientist, as a glance at his most recent work in *Nature* (234, 393; 1971 and page 332 of this issue) shows: with Drs George Oster and Alan Perelson, his interest then was to apply the mathematical techniques of network theory to the description of irreversible thermodynamic systems. It is not often that a man of 58, buoyed up as Katchalsky was not merely by a passion for science but by a deep interest in the organization of science, the relationship between science and the wider world and a deep concern for the political problems of the world as a whole and of his own country in particular, can turn his mind to the radical revision of the branch of science in which he has worked for most of his life. But the particular irony of Katchalsky's death is that he was conspicuous among Israelis for his concern that something should be done to bring about a rapprochement between Israel and its Arab neighbours. If there can have been, at some remote stage in the planning of this bizarre massacre, a calculation that the act would somehow help the Arab cause, Katchalsky's death is by itself a sufficient proof—if one were needed—that the calculation was absurd.

Terrorism is like this. Over the past few years, the bombings in Northern Ireland, mostly by the two wings of the Irish Republican Army, have done more to prevent a settlement of the longstanding dispute between the Catholic and Protestant communities than to demonstrate that the Catholics have a case. Indeed, the point now seems to have been reached when terrorism is an end in itself. If there were at some distant stage lines of argument that may have persuaded its perpetrators that terrorism might serve some purpose—which is not to suggest that these arguments would have been acceptable to rational people—they have long since been forgotten. What can be done to moderate the pandemic of acts of terrorism as varied as the killings recently reported in central Africa, the perennial threat of indiscriminate slaughter in the Middle East, the trouble in Northern Ireland, incidents such as the bombing of the mathematics building at the University of Wisconsin two years ago and, last week, the hijacking of an aircraft in California in an attempt to secure the release from prison of Miss Angela Davis? The short answer is nothing, for there are no reasons to support the notion that all these assaults on the conventions of civilized society are somehow the products of an efficient subversive international organization. Counter-terrorism has no positive and immediate courses of action open to it. The airlines, for example, must search passengers' baggage and then merely hope for the best. Only by a more widespread acceptance of the belief that civilized institutions are civilizing will terrorism, in the long run, be contained.

The letter on page 357 from an international group of biologists quite properly recognizes that there is very little the scientific community can contribute to relief from terrorism, but the signatories are also right in urging that the events last week at Tel Aviv will not be followed by retaliation and counter-retaliation. That would be pointless and in the end would create still further obstacles to a political settlement in the Middle East. So how best to contain the frustration which comes from the feeling that "something should be done"? This is where the proposal that there should be a Katchalsky Memorial Fellowship at the Weizmann Institute has sprung from. For one thing, fellowships are good things anyway. For another, the breadth of Katchalsky's interest would provide a splendid opportunity for recruiting scientists and others of various disciplines to the place to which he was most attached. And there is always the hope that a permanent memorial to Katchalsky in this form would serve as a perpetual reminder of the futility of terrorism. At this stage, the first need is to know what the scientific community thinks of the proposal. Further details of the practical arrangements will be available a week from now.

Standard Literature

THOSE who would standardize the scientific literature will unhappily be buoyed up by the publication, in the past few weeks, of British Standard 4811: 1972, *Specification for the presentation of research and development reports*. The objective is plausible enough. The preamble to the new standard says that since the Second World War, research and development reports have become a distinctive part of the literature of rapid communication in science and technology and goes on to say that steps must be taken to ensure that readers of these reports should be given as much help as possible in understanding them. So far, so good, but whether this need is met by a standardization of the format of research and development reports is another matter. The new standard suggests that authors should first define their objectives in writing a report, gather the information it will contain and then analyse that information. There should follow, the standard says, a section concerned with the conclusions and then a summary. There is a list of detailed headings under which these various components of a research and development report might be included. Nobody will quarrel with the general idea that authors should know what they intend to write before they put pen to paper nor even the suggestion that pages should be numbered (though why the title page of a report should necessarily be called page 1 is entirely another matter). But is it really sensible to claim that the great variety of R and D reports can all fit into the same framework?

Sometimes, for example, research and development reports produced by research institutes and



laboratories are meant to serve the purposes of learned articles in the scientific press—they are means of communicating novel research results to other workers in related fields. There is, of course, an argument that research and development reports, privately or publicly circulated by the organizations from which they spring, should not be used in such a way—why not the scientific journals instead, where reports can be properly refereed before being given wide currency? Other research and development reports are intended not so much as a means of technical communication as devices for persuading managers to follow some recommended course of action, in which case the form of presentation must necessarily be different both from that of the usual scientific article and from the form now recommended for general adoption. Underlying all this, of course, is the general principle that the form of any piece of literature, scientific or otherwise, should be determined exclusively by the immediate purposes it is intended to accomplish. If the new standard were to be taken seriously, this objective would be even harder to secure than it seems to be at present.

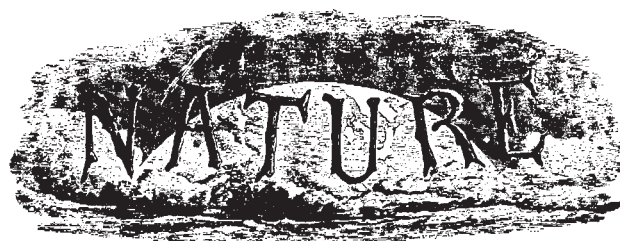
What will the UN do?

THE Secretary-General of the United Nations and several other opening speakers at the Conference this week at Stockholm (see page 303) have done their best to make sure that the problem of the environment will be seen in the perspective to which it belongs. From the start, there has been a danger that too much excitement about the environment might divert attention from other practical needs—the provision of more foreign aid and means of administering it top of the list, disarmament and other belated issues a close second. It is to be hoped that the remainder of the conference will take its cue from these measured statements. It is also important that now and then national governments are required to say what they think of the work at Stockholm in the autumn, they should recognize the importance in the management of the human environment not merely of conservationally inclined measures but also of steps which might be taken still further to improve the use which people are able to make of natural resources. This is why it is particularly apt that Mr Olof Palme, the Prime Minister of Sweden, should have singled out thermonuclear power for particular attention. If people have cause to worry about the exhaustion of natural resources, then surely it follows that steps should be taken to find new ones.

One of the difficulties that will face the United Nations in the autumn is the management of the package of proposals to come from Stockholm, whatever it includes. As things are, several United Nations agencies have an interest in environmental matters. The World Meteorological Office, one of the oldest of them, has for example been coordinating measurements of air pollution for several years and is almost ideally suited to take on the more sophisticated work now necessary to monitor concentrations of carbon dioxide and of the trace materials in the atmosphere. And then there are United Nations organizations with an interest in fisheries—and, by extension, sea pollution—and health. Inevitably, if there is to be a large environmental programme organized internationally by the United Nations, agencies such as the

World Bank, the United Nations Development Programme and several of the regional agencies now scattered about the world will also be involved. In the circumstances, it is hard to see how it would be possible to create an organization equivalent in stature to the large agencies such as the World Health Organization, with its own budget and an enviable degree of autonomy. This is why the best solution is probably to create within the United Nations a coordinating body able to know what work needs doing and can be done and able, where necessary, to commission research and investigation not merely from existing agencies but from outside the United Nations. One of the defects of the present United Nations organization is that it makes far less use than it might of unofficial organizations, universities for example. And to many United Nations officials it will seem ironical that plans are being hatched to spend \$100 million over the next five years on the environment when the United Nations itself is virtually bankrupt.

100 Years Ago



NOTES

THE belief in the safety of Dr. Livingstone caused by the telegrams which we published a fortnight ago, has been strengthened by further intelligence received since our last issue. The following telegram was to hand in London on Saturday last by the Falmouth, Gibraltar, and Malta Telegraph Company :—"Kirk reports from Zanzibar Livingstone safe at Unyanyembe. Visited north end of Tanganyika. Rivers said to flow into Lake Tanganyika. Stanley near the coast with letters. —GOVERNOR." No date is attached to the telegram ; but at the meeting of the Royal Geographical Society on Monday evening, Sir Henry Rawlinson stated that he considered all doubt now to be removed, and that the intelligence now in hand is really authentic. He laid great stress on the well-known cautiousness of Dr. Kirk in receiving and communicating information respecting Dr. Livingstone. The following despatch from the Sultan of Zanzibar to Sir H. Rawlinson was read at the meeting :—"In the name of the Most Merciful God.—To our esteemed friend, Sir Henry Rawlinson.

And it has pleased me much to do that which is considered advisable, and that I am enabled to aid the people in their search for my friend Dr. Livingstone, and I pray God that certain information regarding him may soon be received, and I will give my aid to those gentlemen whom you have sent in attaining their object. And the Consul having requested me to grant the use of my steamer to the above gentlemen to convey them to Mombassa to procure men to accompany their expedition, I have done so, and, please God, I will continue to render assistance to those whom you have sent in your endeavour to explore the mysterious regions of the unknown country, because their object is praiseworthy, and tends to increase our knowledge of what the Almighty has created in these our countries.—From your friend, BURFASH BIN SAYYID.—Dated Zanzibar, the 5th day of Safr, year 1289 of Hejira, corresponding with the 14th April, 1872." Further telegraphic intelligence may be expected at any moment, and should this arrive we purpose next week to resume the present state of our knowledge respecting Dr. Livingstone's safety.

From Nature, 6, 128, June 13, 1872

OLD WORLD

Problems of Development at Stockholm

by our Special Correspondent in Stockholm

THE United Nations Conference on the Human Environment opened this week with the blessing of a royal presence, the sound of the Royal Swedish Orchestra, a cycle of touching songs and a sensible speech by the Secretary-General of the United Nations, Mr Kurt Waldheim. But even Stockholm has its environmental hazards—the first session was delayed for more than half an hour by heavy traffic. And in the press of photographers anxious to catch Mrs Shirley Temple Black—one of the US delegation—Mr Maurice Strong, the secretary-general of the conference, and his entourage were almost forgotten.

Everybody was at pains to thank the Swedish government for setting the conference in train with a resolution in the UN General Assembly four years ago, but Stockholm itself is a mixed blessing, splendid though the city may be. With 1,200 delegates, as many officials and camp followers to match, the place is crowded. Government business must be at a halt, with two government buildings given over to the conference and, when the United Nations is on its own ground at Geneva or New York, there is less likelihood that closed circuit television sets will be supplied with the right pictures and the wrong sound.

Mr Waldheim's chief concern was to emphasise that concern for the environment is not the single most important of present problems nor necessarily in conflict with other good causes, economic development in particular. We should be grateful to the Industrial Revolution, he said—many of us would not have been here without it. "No human being in the past has ever had so many blessings as the average citizen has today in the developed world". But this is only a third of the world's population and even in developed countries there is poverty. So "development is still the highest priority and an un-reached objective". And even though there is an "environmental crisis" which predicates international solutions, the "ugliest of all pollutions" is military, because it is not merely a direct threat to survival but also a drain on resources which might otherwise be spent on the relief of poverty.

Mr Maurice Strong, also anxious not to scorn development, walks a tighter rope. "No growth" is not a viable policy for any country but "we must control and redirect our processes of growth". And because the developing countries "can ill afford to put the needs of an uncertain future ahead of their immediate needs for food, shelter, jobs, education and health care", it will be

for the developed countries to make the most profound changes.

But how serious is the environmental threat? Mr Strong said that "one does not have to accept the inevitability of environmental catastrophe to accept the possibility". "We need subscribe to no doomsday threat to be convinced that we cannot wait for all the evidence to be in." And he argued convincingly that if there may not be an environmental crisis then there is at least a policy crisis—the proliferation of unintended consequences of past decisions.

Mr Olof Palme, Prime Minister of Sweden, took a stronger line on Tuesday—he knows of no evidence that known environmental problems cannot be solved. Development and disarmament are in any case dominating objectives. He welcomed the notion that the conference should be the starting point

for continuing international action such as the proposed monitoring programme, but he asked whether the time had not come deliberately to encourage research and development in fields such as thermonuclear fusion.

The programme mapped out for the conference is formidable by any standards and nobody will be surprised if the agenda next week is either unfinished or skimmed. The plenary sessions consist of statements chiefly about the proposed Declaration on the Human Environment or declarations of what governments are prepared to do. Close on 200 recommendations with international implications have been referred to three parallel committees, one of which spent its first morning on an incomplete discussion of a single issue.

Officials say that this conference differs from previous United Nations conferences in its careful planning. Certainly the Action Plan which forms the basis for discussion is both wide and detailed, and some particular proposals such as that for global monitoring lean heavily on the ICSU document on the subject. Other recommendations, such as that nations which suffer from shifts in foreign trade occasioned by environmental policies should be compensated, have by comparison seemed designed to continue in Stockholm the wrangle between rich and poor countries which brought the UNCTAD 3 conference at Santiago two months ago to disarray. And it is hard to find out precisely how the United Nations would organize the proposed coordination of collections of microorganisms. The preparation, in other words, has been patchy, and the cost of the proposals is being added as an afterthought.

As forecast, the Soviet Union and several other countries, Albania included, have not turned up, but this has not so far cast a pall on the conference. Reports will eventually have to be ratified by the General Assembly of the United Nations, when the Russians will no doubt have their say unencumbered by delicacy about the status of East Germany. Whether that will entail a rehashing of the more controversial recommendations remains to be seen. And will the Soviet Union eventually contribute to the \$100 million voluntary fund which Mr Strong estimates will be needed (over five years) to finance the schemes he has put forward? (The United States has already offered 40 per cent of this and Mr Peter Walker says that the British government will also contribute.)

British Delegation

THE British Delegation to the Stockholm Conference on the Human Environment is led by Mr Peter Walker, Secretary of State for the Environment, assisted by Mr Eldon Griffiths, under-secretary of state at the department. The other members of the delegation include three of the four chairmen of the working parties set up to assess public opinion in Britain and whose reports were published recently, Lady Dartmouth, Mr Dennis Stevenson and Mr Ralph Verney. Mr Anthony Crosland, shadow minister for the environment, is also in the party. The TUC, CBI and GLC are all represented, and the delegation is completed by Professor G. T. Goodman, Department of Botany, University College of Swansea, and Chelsea College, University of London, Mr M. Mackie, a Scottish industrialist, Sir Frank Marshall of the Association of Municipal Corporations, and Mr J. M. Whittaker, deputy chairman of the County Councils' Association. The delegation is supported by a large contingent of civil servants from the Department of Trade and Industry, the Department of the Environment and seven other government departments, together with officials from the Natural Environment Research Council and the Countryside Commission.

As always, the American delegation is the largest of all. Its chairman, Mr Russell Train, the chairman of the US Council on Environmental Quality, cheerfully rides criticisms that it is strong on politics and light on science. He hopes that the conference will come out strongly on issues such as ocean dumping, the hunting of whales and the conservation of wild species. He is less certain when trying to justify the presence of questions of foreign aid and disarmament on the declaration but not on the agenda.

The most contentious of the working groups is that concerned with economic development. The first contentious set of issues was a recommendation that nations should not use policies of environmental protection as an excuse for restricting foreign trade and that, in particular, if exports from developing countries are affected, compensation should be paid. Canada was opposed to this package, chiefly on the grounds that it would be wrong to fossilize tariffs where they are at present. The United States argued in a familiar way that conservation would contravene the GATT rules on discrimination in trade.

The developing countries would have none of this. What is wrong with the original draft? Is not compensation appropriate? Some even wanted to strengthen the recommendation, adding a provision that there should be international standards of environmental management. Japan introduced its declaration that environmental standards are a matter of national sovereignty with some hollow words about steps being taken in Japan to liberalize trade. No doubt with his mind on the way in which American environmental policies may restrict the export of motor cars to the United States, the United Kingdom delegate kept his head low. The outcome will probably be a United Nations compromise in which the developing countries recognize that the only resolutions likely to succeed are those which have the backing of the great powers.

Other committees have been less contentious and in any case Stockholm is well provided with side shows. At what is called the Environmental Forum, Dr Barry Commoner complained on Monday that the conference was entirely inappropriate because it had not dealt with the issues underlying environmental troubles—the economy and society. Lady Jackson (Barbara Ward) and Dr René Dubos have given a ringing summary of their new book. The city of Stockholm has arranged pleasure trips for all and sundry to local sewage plants. And there is also plenty of junk literature to throw away. It is something of a puzzle that the United Nations should lend its name to packaged documents labelled “Man’s Home”

produced in collaboration with the Conservation Foundation in the United States. The daily newspaper *Eco* produced by Friends of the Earth and *The Ecologist* magazine is an attempt at serious commentary conspicuous for its failure.

INNOVATION

With Eyes on Japan

BRITAIN has a lot to learn from Japan when it comes to reaping benefits from research and development, according to Dr Ieuan Maddock, chief scientists at the Department of Trade and Industry. Speaking at a conference held this week on industrial innovation, Dr Maddock suggested that Britain should put more effort into what he described as the diffusion part of innovation.

Innovation, he argued, consists of three phases, first the invention, second the marketing and proving of the product, and third its rapid sale and distribution. It is in this last stage where the real profits are made according to Dr Maddock. With the aid of many charts, Dr Maddock demonstrated that Britain holds its own against the rest of the world in research and development. “There is” he said, “nothing to suggest that we are not innovative and we should stop contemplating our navels and worrying about it”.

What Dr Maddock does question, however, is the belief that research and development are significant in themselves, and whether the research and development Britain does is in the right sectors. Japan’s research and development concentrates heavily on socially orientated research and on products for immediate economic development. Both Britain and the United States, however, put large sums of money into defence, atomic and space research. Japan virtually ignores these areas, yet it is Japan which has the fastest rate of economic growth.

The key, Dr Maddock believes, lies in too much emphasis on invention and not enough on investment. If the research and development of a product takes one financial unit, then the product design takes two, the tooling up and machine engineering for it takes a further seven, with start up taking three more units, but full exploitation with all its implied extra costs can cost many units more, and it is in this area—“diffusion”—that Britain is weak but Japan (which tends frequently to buy other countries’ technology anyway) is strong. It is this lack of investment that Dr Maddock sees as one of the factors industry should consider when trying to improve its innovative methods.

Professor Christopher Freeman of the Science Policy Research Unit, Sussex, discussed innovation in small companies. Research that his unit under-

took for the Bolton committee on small companies showed that only 10 per cent of the 1,100 major postwar innovations that the unit studied had been the work of small companies (those employing less than 200 people), and that 80 per cent had been the work of large companies with 1,000 employees or more.

Professor Freeman also pointed out that small companies had a better record in certain industries than in others. In the less capital intensive industries such as scientific instruments, construction, paper and certain types of machine tools, small companies are responsible for 17 per cent of innovations. Clearly, Professor Freeman said, it is important to ensure that these small companies retain their ability to be innovative in those areas in which they are effective.

ISRAEL

Science in Mourning

THE tragic death of Professor Aharon Katzir Katchalsky at Lod airport, Tel Aviv, last week has stunned the scientific world. The letter from Professor Katchalsky’s colleagues and friends on page 257 of today’s *Nature* is one reaction to the tragedy, while reports from the Weizmann Institute tell of the body lying in state at the polymer science department, where Israeli cabinet members, the Japanese ambassador to Israel and all the scientific staff of the institute paid their last respects before the funeral which was held on June 2.

Professor Katchalsky, who was born in Poland in 1913, arrived in Israel in 1925 and in 1940 received his PhD from the Hebrew University in Jerusalem. After post-doctoral work with Professor Werner Kuhn at Basel he returned to Israel, where in 1948 he became head of the Polymer Department at the Weizmann Institute, a position he held until his death.

During the Arab riots of 1938 Aharaon Katchalsky joined the Jewish



underground movement, Haganah, and at one time was their commander in the old city of Jerusalem. Later he again played a prominent part in military affairs, and in 1947 was one of the founders of the scientific branch of the Israel defence forces.

Professor Katchalsky was very much an international scientist and his activities included membership of the International Union of Pure and Applied Biophysics of which he was president from 1964 to 1969 and thereafter an honorary vice-president. Katchalsky was also a fellow of the New York Academy of Sciences, foreign member of the American Academy of Arts and Sciences, member of the council of the European Molecular Biology Organization; member of the council of the International Council of Scientific Unions, and foreign member of the National Academy of Sciences. He was also visiting professor at the University of California at Berkeley during 1967-68.

His scientific activities within Israel were no less impressive and from 1962-68 he was the first president of the Israel National Academy of Sciences and Humanities. In 1954 he was awarded the Weizmann prize, and in 1961 the Israel prize in Exact Sciences. Also in 1952 he was made Professor of Physical Chemistry at his *alma mater*, the Hebrew University.

Aharon Katchalsky was widely recognized as the leading exponent of the application of nonequilibrium thermodynamics to the study of irreversible processes in biophysics and biochemistry, and in particular to the transport of electrolytes, nonelectrolytes and water across the membranes in living tissues. As will readily be appreciated by readers of the book he wrote jointly with Professor P. F. Curran in 1965, which was based on a series of lectures given at Harvard, he was a lecturer of outstanding ability, who could make even the most hardened experimental scientist believe, at least for a while, that he could understand the niceties of modern thermodynamic reasoning. Whether he was concerned with the most rarified theory, or with a practical demonstration of an ingenious contractile motor that he designed on impeccable physicochemical principles, his infectious charm and the elegance with which he could express himself in any one of a host of languages, made his lectures a memorable experience. His fame as a lecturer meant that he was in constant demand throughout the world of biophysics.

During his term as president of the International Organisation (later the Union) for Pure and Applied Biophysics he brought great distinction to this office, and IUPAB is deeply indebted to him for the wisdom with which he steered it during its formative years.

Above all, he was a man of the widest culture and very great humanity. The most heartfelt sympathy of all who knew and worked with him will be extended to his widow and three children. Their loss is shared by the world.

SOVIET SCIENCE

Predicting Earthquakes

from our Soviet Correspondent

EARTHQUAKE precautions and attempts at earthquake prediction are a constant theme in Soviet geophysical research—chiefly because the Soviet Union contains a number of seismically active regions and has frequently suffered considerable earthquake disasters. The frequent occurrence in the Russian press of news items related to this problem—sometimes exotic, like the theory that the radon content of thermal springs increases before an earthquake; sometimes more practical, like the highly publicized appearance of the first tectonic map of the Soviet Union (*Pravda*, June 1, 1971)—indicates how large a part the danger of seismic disturbances plays in the minds of Soviet planners.

One of the major earthquake disasters of this century occurred at Leninakan, in Armenia, in October 1926. Appropriately, Leninakan was chosen for the site of the new Institute of Geophysics and Engineering Seismology, which is now keeping a 24-hour watch on seismic disturbances in Armenia, and is also undertaking a research programme intended to determine the least unstable spots in highly seismic regions. This is not the only such research centre in the Soviet Union—there is another on Sakhalin island in the Kurilo-Kamchatka seismic area of the Pacific coast, and a team from the Institute of Earth Physics of the Soviet Academy of Sciences are also doing field work in the High Pamirs on variations in the electrical resistance of mountain rock to the occurrence of earthquakes. The Leninakan Institute, however, is the base for an especially detailed survey, which will map the seismic activity of Armenia until 1975.

Interviewed in *Pravda* (May 24, 1972), the Director of the Institute, Academician A. G. Nazarov, explained that this is a two-fold programme. On the one hand, there is the stationary recording of the geophysical fields at three observatories—on the Zangezurkii ridge (the site of the 1968 earthquake) and in central and northern Armenia. Observations will be made here on the deep structures and seismicity, and also the thermal, electrical, magnetic and gravitational fields, and deformation of the crust, in order to investigate possible correlations between these factors and seismic activity.

The second part of the programme involves establishing research bases in the

most seismically active parts of the Republic of Armenia and carrying out a large-scale programme of engineering work, designed to facilitate earthquake forecasting. The procedures to be used are described as “a complex of geological and geophysical methods”, and appear to be based largely on magnetic and gravitational observations.

According to Academician Nazarov, a connexion has already been established in the Zangezurkii region between strong variations in the Earth's magnetic field and repeated shocks. Attempts are being made to establish a means of estimating the time of a major shock after the last precursor. So far, it has been established that the stronger the expected shock the earlier the precursor.

Of more immediate value, the data gathered by the institute are being processed by a computer to allow estimates to be made of the necessary strength and shock resistance of new buildings to be erected in the seismic zones of Armenia. Simulation by computer allows the effect of both standing and progressive waves to be considered and for the effect of the relief at the proposed site to be estimated. Large-scale models of multi-storey flats for erection in Armenia, designed in accordance with the new recommendations of the institute are undergoing tests.

The impressive record of work carried out by the institute seems to indicate that Armenia will shortly become a world centre for seismic research. But all is not well. According to Academician Nazarov, there is a serious lack of properly trained personnel, and the local state university (Erevan) “is not guaranteeing” the institute's requirements in this respect. Geophysics and the allied earth sciences play an important part in the directives of the current Five Year Plan. The experience of the Leninakan Institute suggests that the full implementation of their programmes may be curtailed by personnel problems, even in so economically vital a field as seismic research.

Birthday Honours

IN the recent Honours List, the following scientists were made Knights Bachelor. Derek Harold Richard Barton, Hofmann professor of chemistry at Imperial College, University of London; Kingsley Charles Dunham, Director of the Institute of Geological Sciences and Foreign Secretary of the Royal Society; John Arthur Stallworthy, Nuffield professor of obstetrics and gynaecology, University of Oxford; Michael Meredith Swann, principal and vice-chancellor of the University of Edinburgh.

NEW WORLD

Kennedy's Prescription for Civil Science

by our Washington Correspondent

WHILE the Nixon Administration has been busy trying to formulate plans to stimulate industrial research and development and to focus scientific research on pressing national problems, Senator Edward M. Kennedy has been working on a prescription of his own. About a year ago, he introduced into the Senate a bill designed to transfer the thrust of investment in research and development from national defence to civilian science and to help unemployed scientists and engineers convert their skills from weapons production to more peaceful ends. Since then the bill has been revised several times, it has been extended to include research into urban planning and the problems of cities, and it now contains a host of projects which would require expenditures of \$2,000 million over the next three years. If the bill is passed by Congress—which is unlikely this year at least—it would alter the whole shape of science policy in Washington.

Called the National Science Policy and Priorities Act, the bill was passed last week by Senator Kennedy's subcommittee which has oversight over the affairs of the National Science Foundation, and it must now be considered by the full Senate Committee on Labor and Public Welfare before it goes to the Senate itself. Even if the Senate gets round to considering the bill this year, however, it is likely to be stalled in the House of Representatives and almost certainly will not be passed by Congress before the end of the Session—the House Committee on Science and Astronautics, which would take up the bill after its passage through the Senate, has been sitting on a companion to Kennedy's original version for more than a year and shows no sign of taking further action on it. But the bill will undoubtedly pop up again next year, when it can be considered outside the atmosphere of election year politics and it cannot be written off as a non-starter.

The bill is certainly controversial, and has already stirred up considerable debate in the Administration. Even its statement of principles, usually a bland set of verbiage that kicks off every bill in Congress, contains two rather radical propositions, namely that expenditures on science and technology should keep pace with or even outstrip the growth in gross national product, and that federal expenditures on civil science should equal or exceed

expenditures on defence-related science and engineering. Both would represent significant departures from past trends—between 1964 and 1970, for example, expenditures on science and technology grew much more slowly than gross national product, with the result that expenditure on research and development dropped from 3.04 per cent of GNP to 2.79 per cent, and although the Department of Defense has been taking a declining share of the total science budget during the past few years, together with NASA it still accounts for 77 per cent of all federal expenditures on science and technology.

But, while the principles enumerated in the bill are controversial, the means by which they are to be achieved are no less likely to provoke disagreement in the Administration, for they would lead to a fundamental realignment of science policy formulation in the federal government. In short, what is being proposed is that the National Science Foundation should involve itself in much more applied research activities than at present, and that there should be established in the foundation a Civil Science Systems Administration to plan and fund research into urban problems such as waste disposal, transportation and health care delivery. This proposal would eventually make the National Science Foundation the central point for formulating federal policy for applied research as well as basic research, and it would be a significant step towards centralizing such activities and away from the traditional US system in which each agency is responsible for planning research activities related to its own objectives.

Kennedy's bill also attempts to strengthen links between the federal government and state and local governments, chiefly with a view to helping to retrain aerospace engineers and technicians, thrown out of work by cutbacks in the defence budget. And it also proposes that \$200 million should be set aside for long-term loans for unemployed scientists and engineers, to tide them over until they find employment and, presumably, to induce them to stay in the United States.

The National Science Foundation plays a leading role in Kennedy's proposals, and its officials are already on record as opposing them. First, the proposals would give the NSF \$50 million over the next three years for a research programme to identify priority areas for investment in civilian science,

and for technological assessment of the economic impacts of such investment. Second, the Civil Science Systems Administration would be set up as part of the NSF, although its top staff would be Presidential appointments, and it would be given \$1,200 million over the next three years to design and demonstrate systems capable of providing public services. Often referred to as Kennedy's urban NASA, the Civil Science Systems Administration would involve itself in all kinds of projects now being carried out piecemeal by other agencies such as the Department of Transportation and Health, Education and Welfare. Finally, the NSF would be called upon to administer the economic conversion projects designed to help convert unemployed defence and aerospace engineers to new occupations and to organize the distribution of loans to out-of-work scientists.

Why have officials of the National Science Foundation chosen to oppose Kennedy's proposals? During hearings on Kennedy's bill, held by his subcommittee last October, William D. McElroy, then Director of the NSF, said that the foundation's programme of Research Applied to National Needs (RANN) already carries out many of the activities proposed for the foundation, and he questioned the need for setting up a new agency to carry out the civil science systems programme. "Both the foundation's RANN programme and the New Cities programme of the Department of Housing and Urban Development have significant activities under way in this area," he said, "and other mission oriented agencies are conducting research in their areas of specialization."

Underpinning the foundation's reluctance to take on a huge increase in its responsibility for applied research is also the fact that Kennedy's bill would completely alter the NSF's centre of gravity. The foundation is traditionally the federal government's focal point for support of basic science, and it lacks the capability or the mechanism for evaluating the worth of applied science projects. It is still feeling its way with the RANN programme, for example; it has yet to test its expertise in the handling of the new technology incentives programme, and its officials are very sensitive to accusations that the foundation is abandoning its constituents in university basic science departments.

Discussion of Kennedy's bill has so far been limited chiefly to its impact on

unemployment among scientists and engineers. The bill should, however, be seen more in terms of its impact on science policy in Washington, for it represents a fundamental realignment of policy making, by taking responsibility away from mission oriented agencies and placing it in the hands of what would in effect be a department of science and technology. If the bill does no more than stimulate debate on that issue, it will have done a great service to science policy.

Wish list for the 70s

by our Washington Correspondent

ASTRONOMY, like many other branches of science in the United States, has been tightening its belt in the past few years. levelling of financial support, a lull in There has been the familiar pattern of a the funding of new projects and a rapidly increasing production of PhDs. And the situation has been particularly acute in radio astronomy, where virtually no new facilities have been given the go-ahead since the mid 1960s, but where there has been a rapid increase in new ideas and knowledge. But the situation, at least in some branches of astronomy, may be improving—the National Science Foundation has been given the green light for the very large array system of radio antennae (VLA), and a site for the project has already been chosen in New Mexico.

Nevertheless, according to a committee of the National Academy of Sciences, unless the budget for astronomy in the United States increases by at least 5.5 per cent a year in real terms,

during the rest of the 1970s, astronomers will be hard done by. The committee, under the chairmanship of Dr Jesse L. Greenstein of California Institute of Technology, has drawn up a list of eleven projects which it suggests are vital for the health of astronomy in the US, and which would cost a total of \$1,200 million in construction and operating funds during the next decade. The wish list is the product of an exhaustive survey of US astronomy, conducted by eleven panels of scientists and drawn together by Dr Greenstein's committee. (*Astronomy and Astrophysics for the 1970s*, National Academy of Sciences, 2101 Constitution Avenue NW, Washington DC 20418.)

The committee argues for such a budget increase chiefly on the basis that the recent slowdown in funding increases has come at a time when there is a flood of discoveries and new opportunities in astronomy, and where the potential for space based research is rapidly expanding. The situation, the committee believes, "forebodes an imminent crisis in support per scientist trying to do research in astrophysics at a time when the quality, depth of education, and technological skills of those entering the profession are the highest that they have ever been".

At the top of the list of projects are four which the committee believes are essential for the future of astronomy in the United States. They are:

- the VLA, accompanied by a \$25 million increase in funding for medium sized university operated radio telescopes during the next ten years;

- a programme for upgrading all major US optical telescopes with electrooptical detectors and construction of a multi-mirror optical telescope equivalent to a conventional 150 or 200 inch instrument, followed by construction of a similar 400–600 inch telescope. The total in construction money for such optical facilities would be about \$50 million over the next decade.

- a doubling of expenditure on infrared astronomy, to be kicked off by a balloon survey of the sky for objects bright in the far infrared. Such a survey, the committee believes, could be conducted for less than \$200,000, and it suggests a total budget for an expanded infrared programme of \$25 million over the decade, compared with present expenditures of less than \$2 million a year;

- the final essential programme is the high energy astronomy programme that has already been given the go-ahead by NASA. The committee suggests that NASA should now seek authorization for pointable HEAOs to supersede the first rotating HEAO, and that four missions be flown in the 1970s.

These four projects together would take about half the \$1,200 million sug-

gested by the committee as a reasonable budget for astronomy in the 1970s, but they should not be allowed to crowd out the seven other projects assigned a lower priority but which are nevertheless "essential to the health and balance of the total astronomical enterprise". These include construction of a very large millimetre wavelength antenna for interstellar spectroscopy and to study the early evolution of quasars; a doubling in support for aircraft, balloon and rocket based observations, particularly for ultraviolet studies to back up the Orbiting Astronomy Observatory series of spacecraft; increased support for theoretical astronomy, especially for computing facilities, and a programme of optical space astronomy leading to the Large Space Telescope (LST).

The committee also throws its weight behind those who are campaigning to save the Orbiting Solar Observatory series of spacecraft, arguing for continuation of the programme for another six satellites. Unfortunately, however, while the committee's report has been awaiting publication, NASA has already deferred funding on two of the three remaining satellites, and there is even some doubt about the remaining one.

If the committee's recommendations were adopted, they would run up a bill of about \$355 million a year over the next decade, compared with present expenditures of \$275 million. One shortcoming of the report is that it does not predict the likely scientific consequences if the budget for astronomy in the US were to grow more slowly than its recommendations suggest. To judge by past trends, that is more than a possibility.

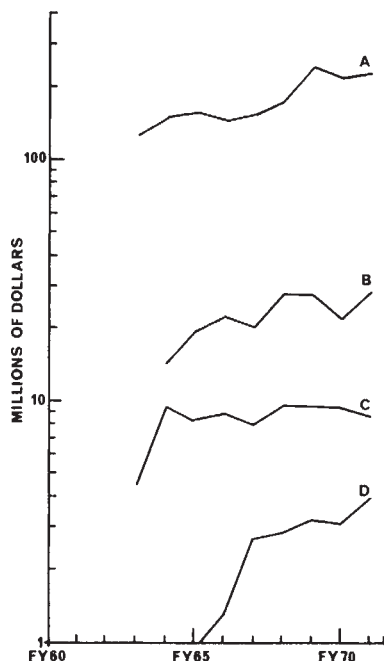


Fig. 1 Federal obligations for basic research in astronomy. A, NASA; B, NSF, including major research instruments (as does NASA); C, Department of Defense; D, Smithsonian Institution.

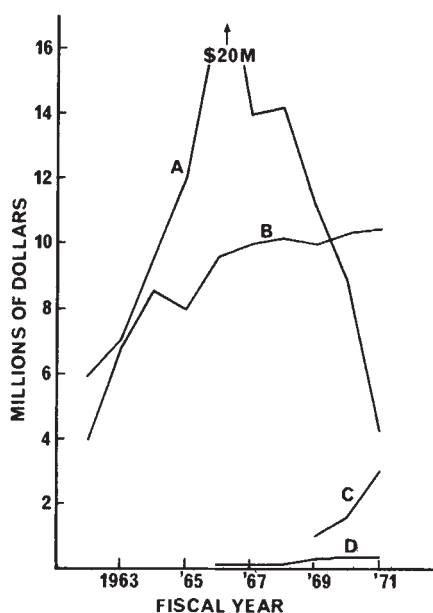


Fig. 2 NASA budgets for specific astronomy programmes which do not involve satellite observatories. A, Launch vehicles; B, sounding rockets; C, airplane observatory; D, data analysis.

SEISMOLOGY

DoD keeps Control

by our Washington Correspondent

THE Department of Defense came close to losing its seismology research programme last week. A little noticed provision of the bill authorizing funds for the Department of State would have transferred the programme from the Pentagon to the Arms Control and Disarmament Agency (ACDA), but the provision was deleted from the bill when it came up in the Senate.

Those who were trying to wrest control of the programme from the Defense Department can, however, claim at least one significant victory. Anxious not to lose the programme, which is designed to provide the theoretical backing for a monitoring system to police a possible treaty banning all testing of nuclear weapons, the Department of Defense has given assurance that it will not be phased out, as many had feared. Instead it is to be broadened in scope and the changes could have a significant impact on seismological research.

Supporters of the bid to transfer the seismology research programme to ACDA, led by Senator Clifford Case, offered an amendment to the authorization bill which was approved by the Senate Foreign Relations Committee. Their argument was essentially that since the Defense Department is concerned chiefly with developing nuclear weapons, it suffers from a conflict of interest in supporting a programme designed to lead to a ban on weapons testing. To illustrate their case, they point out that the Defense Department has allowed funding for the programme to decline steadily since 1963, when it was given \$41.4 million, to less than \$10 million that is being requested for 1973. Underlying this suggestion is the belief that if the programme is transferred out of the obsessive secrecy that surrounds all the Pentagon's programmes, its future will be assured and its results will be more readily available to Congress.

But those arguments fell flat last week when the authorizations bill came up for debate in the Senate, and an amendment designed to preserve the *status quo* was passed by 45 votes to 34. Proposed by Senator Peter Dominick, and co-sponsored by a handful of the more conservative members of the Senate, the amendment carried the day for two chief reasons. First, both the Department of Defense and the ACDA were opposed to the transfer—indeed, Gerard Smith, Director of ACDA and the chief US representative at the SALT talks, said in a letter to Senator Case, "I do not see compelling reasons favouring the transfer you propose and I foresee many problems". And, second, Senator Dominick made great play of the fact

that ACDA has not been responsible past, and lacks the expertise for such for extensive scientific research in the activities.

Another trump card that Dominick and his supporters had to play was a letter from Dr John S. Foster jun, director of Defense Research and Engineering, to the chairman of the Senate Armed Services Committee, John Stennis. Foster said that the Department of Defense will continue to fund the seismological research programme and will reorient it so that "the thrust of our future research will be concerned with verifying a comprehensive test ban treaty".

The programme outlined in Foster's letter will consist of three parts. The first will be concerned with pinpointing geographical areas that may give rise to anomalous seismic events, "with a view to reducing the false alarm problem". The second will be aimed at designing a worldwide network of seismometers to monitor seismic events and, presumably, to police a test ban treaty if one were concluded, and the third part of the programme will be concerned with the problem of evasion; it will include research aimed at finding seismological techniques to detect clandestine tests, and it will also be concerned with considering "the impact of non-seismic means to deter the use of potential evasion techniques".

The announcement that the seismic research programme will be broadened to look at possible anomalous areas may provide something of a shot in the arm for seismology, for the idea is to spread the net wider than in the past. Previous

research has been focused chiefly on the geography of the USSR, but according to one official of the Defense Department concerned with the programme, the research will now be extended to other areas, although no firm decision has yet been taken as to which areas. The commitment to design a worldwide monitoring system is also welcome news for proponents of a comprehensive test ban, for it indicates that the Department of Defense is sufficiently impressed with the results of past research on monitoring techniques to apply them to a policing system.

But perhaps the most far-reaching of the plans outlined in the letter is the suggestion that the US government is willing to reconsider the impact of non-seismic means to deter potential evasion techniques. The United States government has always insisted that a comprehensive test ban treaty should include provision for on-site inspection, while the USSR has always bucked such a suggestion, and consequently negotiations have not gone past this hurdle. Recently, however, there have been signs that the US government may be reconsidering its position—Dr Stephen Lukasic, director of the Defense Department agency which controls the seismic research programme, told the Joint Committee on Atomic Energy last October, for example, that on-site inspection probably would not detect evidence of a small test. Foster's remark about considering the impact of non-seismic detection techniques is therefore being interpreted as further evidence of rethinking in the Defense Department about on-site inspection.

Short Notes**FDA Takes All**

THE Division of Biologics Standards is to be transferred lock, stock and barrel from the National Institutes of Health to the Food and Drug Administration on July 1. The announcement of the transfer, made last week by Elliot L. Richardson, Secretary of Health, Education and Welfare, came as something of a surprise in view of his earlier announcement that only the DBS's regulatory functions would be transferred (see *Nature*, 237, 66; 1972), and it means that the NIH will lose all the research on new vaccines and vaccine techniques. The proposed total transfer of the division is also surprising in view of the fact that the DBS has been criticized in the past for conflict of interest since it both develops and regulates vaccines; separation of the agency's research and regulatory activities would therefore have circumvented such accusations. But keeping them together should ease the transfer of research results to the regulatory functions.

ZPG at Last?

THE birth rate in the United States fell during the first three months of 1972 to its lowest point in history. At 15.8 births per 1,000, the birth rate is considerably lower than it was a year ago, and it comes close to the level required for zero population growth. Liberalized abortion laws and the economic climate may have depressed the birth rate, but even if the effect is not merely a short-lived phenomenon, it will take forty or fifty years before the population actually reaches a stable size.

NSF Official leaves for Texas

DR LOUIS LEVIN, former Assistant Director of the National Science Foundation for Institutional Programmes, is leaving the foundation to become a special consultant to the Texas Tech University. Levin, who resigned last month as assistant director of the NSF because his budget had been contracting for the past few years, was connected with the foundation for sixteen years.

NEWS AND VIEWS

Submarine Lithification of Ancient Limestones

THE discovery of hard, rocky sea beds formed by recent underwater cementation of carbonate sediments (enclosing pottery) is the basis for new thinking on the early lithification of ancient limestones. Limestones are almost pure calcium carbonate sediments (for example, grains of coral and shell debris, oolite) which have been lithified by precipitation of crystals of cement in their pores and by wet recrystallization (calcitization). During the past decade it has been found that extensive lithification can take place in two environments: first, in seawater just below the sediment surface; and, second, in freshwater in sediments exposed to rain and soil waters. Attempts to understand the lithification environments of ancient limestones have led geologists to look at the ways in which the physico-chemical parameters of these environments can be recorded in limestones which were lithified millions of years ago.

There is good evidence that ancient marine limestones began as sediments which were mineralogically similar to those of today. Modern marine carbonate sediments are composed largely of two polymorphs of calcium carbonate—aragonite (orthorhombic) and calcite (trigonal). In aragonite—corals, molluscs and suchlike—strontium substitutes for some calcium; for example, in some living corals strontium is 0.9–1.5 mole per cent (B. F. Spiro, *Bull. Geol. Soc. Denmark*, **21**, 1; 1971). In calcite skeletons—for example, echinoderms, many foraminifera and calcareous algae—substitution of magnesium for calcium is considerable (K. E. Chave, *J. Geol.*, **62**, 266; 1954), chiefly in the range Mg 12–17 mole per cent (the so-called high magnesium calcites). It is known also that lithification of modern carbonate sediments in seawater involves the precipitation of these same solid phases, aragonite and a range of high magnesium calcites (various authors, *Sedimentology*, **12**; 1969). In contrast, lithification in freshwater yields cement crystals of low magnesium calcites (< 5 mole per cent Mg) and causes alteration of pre-existing aragonite and high magnesium calcites, both grains and cement, to low magnesium calcites, including loss of strontium and magnesium (G. M. Friedman, *J. Sedim. Petrol.*, **34**, 777; 1964; W. H. Harris and R. K. Matthews, *Science*, **160**, 77; 1968). The ancient limestones are now purely low magnesium calcite, having undergone final alteration in freshwater. After such a history, what record can these limestones retain of any earlier submarine stage of their lithification?

The late subaerial stages of lithification must, of course, take place by dissolution-precipitation processes in a more or less open freshwater system, because strontium and magnesium are lost. With the microscope it can be seen that two dissolution-precipitation processes have acted (R. G. C. Bathurst, in *Approaches to Paleoecology*, Wiley, 357; 1964): one process involves total dissolution; the other consists in molecule by molecule replacement by dissolution of aragonite and precipitation of low magnesium calcite. Where, for example, this replacement process (calcitization) acts on an aragonitic mollusc shell, relics remain of the intercrystalline sheets of organic matrix, because, as the exchange process is diffusional across a water film, such large objects cannot be flushed

by the pore water. In cements there may be a similar preservation of impurities, originally trapped in the interstices of radiate clusters of aragonite crystals. These impurities can be expected to survive calcitization of the aragonite crystals as radiate patterns of oriented inclusions in the coarser secondary mosaic of low magnesium calcite crystals. Such relic patterns are being found by A. C. Kendall and M. E. Tucker (University of Reading), who postulate a primary aragonitic, and therefore marine, origin for what would have seemed, a few years ago, an obvious subaerially precipitated, low magnesium cement.

In the calcitized mollusc shell the crystallographic orientations of the new low magnesium calcite crystals mimic to some extent the original aragonite orientations. A more exact crystallographic replication occurs in the alteration of echinoderm tests from high magnesium to low magnesium calcite. How this copying takes place is arguable—though it is known in metamorphic rocks (G. Voll, *Liverpool Manchester Geol. J.*, **2**, 503; 1960)—but it should be expected to occur also as a result of calcitization of aragonite cement. This mimetic crystal fabric is just what Kendall and Tucker believe they are finding in low magnesium calcite cements, which must now be supposed began as aragonite. In a recent issue of *Nature Physical Science* (**237**, 43; 1972) R. C. Lindholm described fibrous low magnesium calcite crystals in veins where each fibre is composed of numerous subcrystals with optic axes across the fibre. This anomaly, allied to the unusually high content of magnesium in the low magnesium calcite, suggests derivation from an initial vein precipitate of high magnesium calcite. The radiate pattern of optic axes in radial low magnesium calcite crystals (R. G. C. Bathurst, *J. Geol.*, **67**, 506; 1959) has long been suspected to have an aragonite origin: the work of Kendall and Tucker supports this hypothesis.

P. W. Choquette's analysis (*Science*, **161**, 1130; 1968) of carbon and oxygen isotopes in a finely crystalline low magnesium calcite limestone, partly recrystallized, revealed a positive correlation between the degree of wet recrystallization and the proportions of the lighter isotopes (also C. A. Ross and S. Oana, *J. Sedim. Petrol.*, **31**, 231; 1961). Extrapolation backward of Choquette's data, to the cemented but otherwise unrecrystallized limestone, gave isotopic ratios appropriate to cementation in seawater.

Where the primary solid phase was low magnesium calcite, as in the Chalk of northern Europe, it will have remained unaltered so that its oxygen isotope ratio can even now be used to determine the original ambient temperatures of precipitation (H. A. Lowenstam and S. Epstein, *J. Geol.*, **62**, 207; 1954). Replacement of aragonite and high magnesium calcites by low magnesium calcite, on the other hand, is accompanied by diffusion of ions in freshwater and bulk transport during fluid flow and there is likely to be a substantial but not necessarily total reorganization of the ionic and isotopic content of the original material. It is fortunate that once a limestone has become low magnesium calcite with a porosity of 3 per cent or less, it can resist further changes in the low P–T environment.—From a Correspondent.

EARTH STRAIN

A Growth Industry?

from a Correspondent

THERE have been many measurements of strain near the surface of the Earth (particularly strains and tilts caused by tides in the Earth), but the contribution of this work to the detailed knowledge of the Earth and earthquake sources has been small. This situation should now change as the number of strainmeter installations multiply and work begins on the interpretation of the results. At least these were the two main impressions given by the contributors to the discussion meeting on the measurement and interpretation of strain at the Royal Society on May 11 and 12.

The principal reasons for the renewed interest in strain seem to be: first, many new instruments are available for strain measurement, particularly cheap and portable strainmeters; second, strain measurement may provide the key to earthquake prediction (another growth point in the Earth sciences); and, third, abrupt change in strain and tilt (strain and tilt steps) associated with earthquakes can apparently be recorded at long range.

Dr G. C. P. King (University of Cambridge) described the development of strainmeters from the primitive attempts of Milne in 1888 using a 1 m steel bar as a length standard to the modern interferometer strainmeters which use laser beams tens of metres long as a standard. These laser strainmeters, however, have the disadvantage that they are expensive and not readily portable though they are highly sen-

sitive. Dr J. Levine (University of Colorado) described an experiment using such an ultra-sensitive strainmeter to search for gravity waves and showed that if any such waves exist they must produce strains of less than 3×10^{-17} .

Dr King suggested that the most important advance in recent years was the development of cheap portable wire strainmeters which give satisfactory results if simply installed in a shallow trench in sand or clay. Rigid attachment of the strainmeter to solid rock (usually in a tunnel or vault) is not required. Several such meters could thus be installed around any chosen site and the outputs of all the instruments combined. This procedure should reduce local site effects which can be large, as was demonstrated by Dr W. H. Ward (Building Research Station) who described the large, local ground strains that can be induced by civil engineering projects.

Mr R. V. Allen (National Center for Earthquake Research, California) also described a portable strain measuring instrument—a tiltmeter, suitable for installing in boreholes, which uses a simple pendulum with a capacitance transducer and has about the same sensitivity (10^{-8} radian) as larger and more cumbersome liquid level tiltmeters. Again, rather surprisingly, this instrument gives reliable results when simply installed in a sand dune.

Many strain and tiltmeters of a wide variety of designs are already in operation in California as part of a large scale programme aimed eventually at earthquake prediction. Dr R. G. Nason (Earthquake Mechanism Laboratory, San Francisco) listed five universities

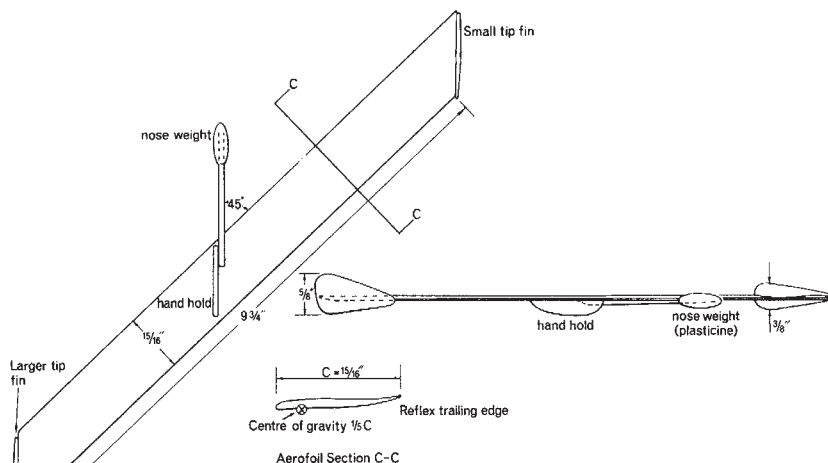
and three government agencies currently working in the California area and one gained the impression that the San Andreas fault was slowly being stitched together by instruments spanning the fault. Dr Nason showed that sensitive instruments are not required to demonstrate fault movement in this part of California; man-made objects such as pavements and concrete lined ditches show effects indicating creep rates of the order of 10 mm/year.

One of the interesting features shown by this instrumentation, however, is the occurrence of "creep events"—periods of relatively rapid creep along the faults. Dr C.-Y. King (Earthquake Mechanism Laboratory, San Francisco) described these creep events as similar to seismic faulting with failure propagating along the fault but with very low velocity (10 km/day or less at the surface) and maximum slip velocities of 0.1 to $10 \mu\text{m s}^{-1}$. These velocities are much lower than those in seismic faulting and so they do not excite seismic waves. Similar, slowly propagating tilts were described by Professor K. Kasahara (University of Tokyo), who reviewed current earthquake fault studies in Japan with particular reference to work in the vicinity of the epicentre of the Great Kanto earthquake of 1923. Current measurements of strain accumulation in this area are consistent with the fault model proposed from seismological studies.

The most controversial observations described at the meeting were the tilt and strain steps associated with earthquakes and recorded at long range (up to 9,000 km). Nobody seemed to doubt that strain steps occur. Dr J. Berger (University of California, La Jolla), for example, recorded such a step from the recent destructive San Fernando (California) earthquake, but the recording station was only 200 km from the epicentre; no such steps had been recorded at this station from earthquakes at much greater distances. Dr E. Berg (University of Alaska), however, reported tilt steps recorded in Alaska from earthquakes in New Guinea, Chile and the Solomon Islands. These tilt steps seem to propagate with velocities of around 3.0 km s^{-1} and less and cannot, according to Dr Berg, be explained on purely elastic theory. They may instead propagate as plastic waves.

In discussion Professor F. Press (Massachusetts Institute of Technology) said that if these tilt steps are real (and not artefacts) they implied that the area of movement on the fault plane during an earthquake is much larger than the studies of conventional seismic signals indicate. At the current rate at which strain and tiltmeters are being installed it should not be long before this question of strain and tilt steps recorded at long range is resolved.

Swing Wing or Swivel for SST?



THIS is R. T. Jones's plan of a model to test the feasibility of a new concept for a supersonic aircraft, which was described last week in *Nature* (237, 252; 1972). The asymmetrical orientation of the wing in the model, which should be made from 1/16 inch balsa wood, is that envisaged for supersonic flight. At take off an aircraft built in this way would have its wing perpendicular to the fuselage.

BACTERIA

Mortar in the Wall

from our Molecular Biology Correspondent

SOME of the strangest specimens in the biochemical bestiary are to be found in the bacterial cell wall, and none is more remarkable than the lipoprotein-murein from *Escherichia coli*, the structure of which has been described by Braun and Bosch (*Proc. US Nat. Acad. Sci.*, **69**, 970; 1972). The murein, or peptidoglycan, is a network of polysaccharide chains, cross-linked by short peptides, which is sandwiched between the outer wall of the cell coat and the innermost layer, the cytoplasmic membrane. Covalently attached to the murein at intervals are chains of lipoprotein, which, it has been suggested, are basic building blocks of the cell wall. The lipoprotein is released from its matrix on degradation with lysozyme, and it turns out to contain fifty-seven amino-acids.

Braun and Bosch have determined the sequence, which reveals a remarkable gene duplication phenomenon—at the N-terminus are two serine residues, which are followed by a sequence of fourteen amino-acids that is then repeated almost precisely; there are only five conservative substitutions, three of which occur at the very ends of the homologous tracts. This is followed by another substantial repeat of part of the same sequence, with residues 32–38 almost identical to 25–31. Thereafter some more short homologous tracts can be constructed if several deletions are allowed, and the chain ends in a little cluster of basic residues and a tyrosine.

A scrutiny of the sequence reveals the presence of hydrophobic residues at alternating intervals of three or four amino-acids. This is a highly characteristic arrangement for α -helices in globular proteins, with the hydrophobic side chains lying along one side of the cylinder. That the chain is indeed α -helical is not yet established, but is clearly to be expected, and one might suppose also that its amphiphilic character might place it at an interface between polar and non-polar media.

The point of attachment of the lipid to the murein was established from the sequence of a lipopeptide found in proteolytic digests of the lipoprotein, and turned out to be the N-terminal serine residue. The lipid, which contains no glycerol or phosphorus, and is predominantly palmitic acid, is apparently attached by way of a string of ester and amide linkages to the α -amino group. The nature of the link between the lipoprotein and the murein was also determined. Pronase digestion, followed by partial acid hydrolysis of the murein, generated peptides containing arginine followed by lysine, which could be uniquely located at the

C-terminus of the sequence. The ϵ -amino group of the C-terminal lysine is linked to one of the carboxyls of diaminopimelic acid, a species that occurs in the peptide cross-links of the murein. The diaminopimelic acid is joined by a dipeptide, glu-ala, to an N-acetylmuramic acid residue of the polysaccharide chain. The protein chain, depending on whether it is in fact α -helical, could project at least the greater part of the way through the cell wall, and the lipid at its extremity could then even be rooted in the membrane bilayer.

With the possibility in mind that the peptidoglycan controls the shape of the bacterial cell, Henning *et al.* (*Europ. J. Biochem.*, **26**, 570; 1972) have examined a mutant of *E. coli*, which, within a defined range of magnesium concentration and temperature during growth, can assume a spherical rather than the normal rod-like shape. Analysis of the murein in terms of its concentration, composition and degree of cross-linking showed no differences between spherical and rod-shaped cells, within the limits of precision, and neither was there any measurable difference in the lipoprotein fractions. It seems, therefore, that the morphological anomaly is unlikely to be related to aberrations in the murein. The outer membrane is also not a good candidate, especially because anomalous response to disruptive reagents is observed also in spheroplasts of the abnormal cells. In this case then it seems that the abnormality resides in the cytoplasmic membrane and Henning *et al.* provide some evidence to this effect.

It may be remarked that earlier work from Strominger's laboratory on a different organism, also capable of growing in rod or sphere forms, did show a difference in the murein: in the spheres the polysaccharide chains were shorter, and the peptide cross-links longer, and contained glycine residues, which are not found in the rods. Henning *et al.* do not believe, however, that the murein can govern the overall shape, and promise evidence to support this allegation in the future. They suggest that the alteration of the murein is only one biochemical by-product of the mutation.

Warth and Strominger (*Biochemistry*, **11**, 1389; 1972) have found that considerable differences exist between the mureins from spores and vegetative cells of *Bacillus subtilis*. Not only are a large proportion of the muramic acid residues in their δ -lactam forms (giving the effect of two fused rings), but others bear N-acetyl groups and alanine residues as substituents. The cross-linking density is low, and in consequence of this and of the pendant seryl residues, the concentration of free carboxyl groups is distinctly high. This confers on the spore surface a more polyacidic character than that of the vegetative cells. The authors' suggestion is that an increase in ionic strength will consequently elicit contraction which will also be favoured by the higher elasticity resulting from the paucity of cross-links. Contraction will be accompanied by dehydration, and this, it is thought, is in turn related to the temperature resistance that characterizes the sporulating state.

Rifampicin and Adenovirus Oncogenicity

THE antibiotic rifampicin has an extraordinarily wide spectrum of biological activity; it not only blocks transcription in bacteria but also inhibits the replication of trachoma agent, the replication of vaccinia and other viruses including adenoviruses, and may influence the transformation of cells by the RNA tumour viruses. Furthermore, as Toolan and Ledinko show in *Nature New Biology* next Wednesday (April 14), rifampicin also affects the induction of tumours in baby hamsters by adenovirus 12.

Toolan and Ledinko, intrigued by Diggelman and Weissman's claim that rifampicin suppresses transformation of chick fibroblasts by Rous sarcoma virus, performed a set of experiments in which they injected newborn male and female hamsters with a dose of oncogenic adenovirus 12 and followed it with a series of injections of rifampicin at one of two concentrations. Control animals were injected either with the virus alone or with the drug alone or

with saline. The number of animals of both sexes subsequently developing tumours and the time at which tumours first become detectable were then measured.

Toolan and Ledinko's results reveal, first, that female hamsters are significantly more susceptible to the oncogenic action of adenovirus 12 than are their male litter mates, and rifampicin if anything enhances the susceptibility of the female hamsters. By contrast, however, rifampicin, even at the lower of the two doses used in these experiments, significantly reduces the incidence of tumours in baby male hamsters inoculated with adenovirus 12; the tumour incidence in the treated males was 9 per cent compared with 32 per cent in the untreated controls.

Why female hamsters are more susceptible to the oncogenic action of adenovirus 12 than male hamsters and how rifampicin inhibits tumour induction in the males are at present open questions.

PROTEIN SYNTHESIS

Doing without Cells

from our Cell Biology Correspondent

THE development of cell free systems derived from eukaryotic cells which support *in vitro* efficient translation of added messenger RNAs is currently a major occupation of many cell biologists. There are, of course, numerous interesting questions which no doubt will be swiftly answered once such systems become available. And to judge from the contents list of the latest issue of the *Proceedings of the National Academy of Sciences*, which is always a reliable barometer of the fashionable areas of research, appreciable progress is being made towards the goal of a mammalian cell free system which is as efficient as that derived from *Escherichia coli*.

Metafora, Terada, Dow, Marks and Bank (*Proc. US Nat. Acad. Sci.*, **69**, 1299; 1972), for example, report that material washed from rabbit reticulocyte ribosomes by 0.5 M KCl, when added to a cell free system derived from Krebs tumour cells, enhances by a factor of some five to ten-fold the efficiency of translation of globin mRNAs in 10S RNA preparations obtained from rabbit, mouse or human reticulocytes. When supplemented by these reticulocyte ribosome washings the Krebs cell free system will incorporate 80 pmol of leucine into globin for every 1 pmol of 10S reticulocyte RNA added. These results mean that this cell free system provides an assay for globin mRNAs which is not only almost as efficient as the frog oocyte system pioneered by Marbaix, Gurdon and their colleagues but also is a good deal more familiar to most biochemists.

The stimulation of translation by the KCl washings of reticulocyte ribosomes is dependent on the addition to the system of mammalian tRNAs and these cannot be replaced by tRNAs from *E. coli*. Presumably, therefore, during the preparation of the Krebs cell system endogenous tRNAs are destroyed, and it seems most likely that the washings from the reticulocyte ribosomes include initiation and/or elongation factors which in the unsupplemented system may be at limiting concentrations. These factors from reticulocytes are not, however, specific for the translation of globin mRNAs, for they also enhance the translation of endogenous messengers and, for example, added encephalomyocarditis virus RNA.

There seems little doubt that in eukaryotic cells, as in bacteria, translation depends on factors which catalyse chain initiation and elongation, and Moon, Redfield and Weissbach (*ibid.*, 1249) describe experiments involving the molecular sieve chromatography of

elongation factors (EF1_A and EF1_B) obtained from calf brain. Factor EF1_A, a multimeric molecule with a molecular weight in excess of 150,000, apparently interacts with GTP to form an EF1_A-GTP complex which in turn will form a ternary complex with amino-acyl-tRNA (Phe-tRNA). During the formation of this ternary complex the elongation factor probably dissociates to yield the lower molecular weight (60,000–80,000) species EF1_B.

Weissbach and his colleagues speculate that as the tRNA binds to the ribosome the aminoacyl-tRNA-EF1_B-GTP complex dissociates and the GTP is hydrolysed to GDP. The liberated EF1_B-GDP, by reacting with GTP, might then be reconverted to EF1_A-GTP. If this scheme proves to be correct—at present it is in many respects speculative—one can conclude that eukaryotic factor EF1_A is functionally equivalent to the two bacterial elongation factors, Tu and Ts.

Levin, Kyner and Acs (*ibid.*, 1234) meanwhile have been investigating the initiation of translation of another set of mRNAs translated in eukaryotic cells—the messengers transcribed off reovirus double stranded genomic RNA. As Levin *et al.* report reovirus mRNAs of all three size classes, which can be made *in vitro* by the endogenous transcriptase present in reovirus cores, form initiation complexes with rat liver 40S and 60S ribosomes and rat liver (35S) Met-tRNA_i after mild treatment with formaldehyde which presumably partially denatures the reovirus RNAs to expose the ribosome binding sites. Furthermore, when puromycin is added, a peptide bond is formed which links methionine to puromycin.

By contrast, rat liver (35S) Met-tRNA_m does not form an initiation complex with reovirus mRNA and rat liver ribosomes until supernatant factors are added, and even in the presence of these factors the formation of the complex can be blocked with fusidic acid at concentrations which do not inhibit formation of initiation complex with (35S) Met-tRNA_i. These data strongly indicate that reovirus mRNAs have AUG initiation codons which are specifically translated by Met-tRNA_i and apparently Levin and his colleagues have used their system to prepare reovirus polypeptides with labelled 5' amino termini.

SOIL SCIENCE

Artificial Conditioners

from a Correspondent

THE first international meeting to be devoted to current work on soil conditioners was held at the University of Ghent (Belgium) from April 17–21. The meeting was the result of renewed interest on soil conditioning, particularly in those parts of the world where the traditional methods of aggregating and stabilizing soils naturally—for example by the use of grass-clover leys—cannot easily be applied.

The first synthetic chemical conditioner, krilium, was introduced in 1952, and some of the early work associated with this substance was reviewed by Professor W. C. Moldenhauer (Iowa State University). Unfortunately the soil conditions necessary for effective action of conditioners were not properly understood in the early days. From time to time excellent results, in

Exercise in Quantum Biochemistry

IN spite of the enormous number of published calculations—usually of a rather simple kind—of charge distributions and other parameters in physiologically active molecules, it is still far from clear that the results lead to realistic explanations of their behaviour. New and Richards, in next Wednesday's *Nature New Biology* (June 14), have attempted a critical test of the ability of molecular orbital theory to represent a purely electrostatic interaction.

New and Richards looked for a system in which the energy of a molecular interaction can be graded through a series of compounds, and in which all energetic contributions other than the coulombic can be disregarded. Such a system is the set of quaternized trimethylammonium compounds, which interact as haptens with antibodies, and were studied in 1946 by Pauling and his

associates. It is assumed that the quaternary ammonium salt interacts electrostatically with a negative centre in the antibody. The interaction energy should then be linearly related to the magnitude of the charge density on the nitrogen, if the entropy of association is the same for all members of the series.

Using the extended Hückel molecular orbital method, New and Richards find that the charge density on the nitrogen varies from a value of about 0.13 when the quaternizing group is *p*-aminobenzyl to about 0.17 when it is α -naphthyl. The calculated results for a series of eight such haptens show a reasonably good correlation with the measured free energies of association. New and Richards regard this as a favourable indication of the usefulness of molecular orbital calculations in accounting for the activities of simple compounds.

terms of increased crop yields, were obtained (chiefly on horticultural crops) but they were erratic.

The development of current understanding of the mode of action of different soil conditioners was described by Professor D. J. Greenland (University of Reading). The characteristics of structures to be conditioned were outlined; for example, pores which are large enough for bacteria to enter but which will also hold "available" water must be stabilized. Dr. F. De Bisschop (University of Ghent) described his work on the mechanism of the binding process between soil particles with emulsions based on products of the petroleum industry.

One important aspect stressed at the conference was the need for a multi-disciplinary approach if problems concerned with the development of good stable structures were to be solved. The help that organic chemists could give was illustrated by Professor N. Schamp's (University of Ghent) contribution on the physico-chemical interaction of polyacrylamide and clay particles. Emphasis has now shifted away from such conditioners as hydrolysed polyacrylonitrile to polyacrylamide and polyvinyl alcohol. Polymers need to be sufficiently small to enter the pores of the soil which requires stabilization.

The need for producing conditioners which are not harmful to the environment was stressed, and Professor Schamp described the work of his department on checking potential conditioners for carcinogenicity.

If work on conditioners is to be developed it is necessary to understand what happens when soil structures break down in field conditions. Professor G. Bolt (State Agricultural University, Wageningen) gave an outline of theoretical aspects of swelling and other destructive forces on soil aggregates and was followed by Dr F. Koenigs of the same university, who talked about practical aspects of structure deterioration.

The structure of the soil determines water infiltrability and storage. If the structure is unstable, particularly at the surface, infiltrability is reduced, runoff is increased and erosion either commences or is increased. Dr G. Vachaud (University of Grenoble) described sophisticated methods for continuous assessment of soil moisture in the field and was followed by Professor W. H. Gardner (Washington State University) on erosion control and moisture conservation in dryland farming. Professor D. A. Farrell (Iowa State University) discussed the effect of soil infiltration and runoff in crusted and uncrusted soils. An advance would be made if conditioners could be used in small amounts for treating the "skin" on soils tending to crust so that instead of runoff water movement into them

could occur but would still allow seedling emergence.

Mathematical models were discussed by several speakers, who included Professor W. R. Gardner (University of Wisconsin) on prediction of moisture transmission in conditioned soils; Professor G. Van Steenkiste (University of Ghent) on the study of moisture flow by hybrid computer; and Professor J. Gani (University of Sheffield) on mathematical models in agriculture.

At the final session an attempt was made by Professor W. R. Gardner to draw together the wide range of topics discussed. One of the reasons for the loss of interest in soil conditioners in the 1950s was their high cost. Being relatively complex molecules, the cost of conditioners of the type at present under investigation must remain high. Evidence was produced during the meeting that it may be possible to stabilize soils with much smaller amounts of material. At present, the current approach in the field is to concentrate on relatively small scale problems—for example, stabilization of road banks during the construction of new motorways and the control of sand dunes in semi-urban areas where land values are high. By concentrating work in this much more limited field, it is possible to consider products which are much more costly than are normally acceptable in agriculture.

PLATE TECTONICS

Lithospheric Tearing

from our Geomagnetism Correspondent

As the spreading north Pacific approaches the Aleutian Island arc it is presented with a trench which is generally convex towards the south. In greater detail, the Aleutian trench appears as three almost linear segments with two fairly abrupt changes in direction. Thus at about 180° longitude, just south of the Bowers Ridge, a 40° change in the trend of the trench axis separates the Western and Central Aleutians; and at about 170° longitude a rather smaller change of about 20° marks the boundary between the Central and Eastern Aleutians. In view of these trends, an obvious question to ask concerns the nature of the tectonic processes occurring at the two discontinuities. In particular, how does the spreading lithosphere react as it reaches, and descends into, a trench which presents an angular, as opposed to a linear, front.

According to Abe (*Earth Planet. Sci. Lett.*, **14**, 428; 1972), seismological evidence suggests that at the 40° discontinuity, at least, the lithosphere bends and tears. The basic data to support this conclusion come from an analysis of the travel times from the Longshot underground nuclear explo-

Action of Calcium in Muscle

IN next Wednesday's *Nature New Biology* (June 14), Ashley and Moisesescu present a model for the action of calcium ions in controlling tension in muscle. The development of tension at relatively large calcium concentrations and over short time intervals depends on the square of the calcium concentration, and it is inferred that the process involves two distinct calcium sites. Earlier evidence on the rate of development of tension also indicated that two separate slow processes contribute to it.

Ashley and Moisesescu now examine the fit of available data to simple equations based on the following models: simultaneous binding of two calcium ions in a slow reaction, followed by a further slow rate process; separate binding of the two calcium ions, that of one being contingent on prior binding of the other; separate binding of two ions at different sites that affect the same functional unit of the myofibril.

The first scheme provides a relatively poor fit to biochemical and binding data. The third model is treated in detail, and gives a satisfactory fit of the transient kinetics and also the steady-state characteristics of the ATPase.

Under narrowly defined conditions the ATPase shows an increase over a range of calcium concentration which is tenfold greater than normal, suggesting an uncoupling of the two binding processes, and corresponding predictions from the model are borne out. The quantitative relation between free calcium concentration and the luminescent response of aequorin, which is used to assay it, is also considered. The treatment indicates that the isometric tension can under all conditions be determined directly by the concentration of free calcium ions.

In the same issue of *Nature New Biology*, Endo describes the activation of skinned muscle fibres by low concentrations of calcium ions. He has found that the development of isometric tension under these conditions can be made to increase when the fibre is stretched. Three reasonable explanations for this finding are offered: increased binding of calcium to troponin when the length is greater; stronger interaction between thick and thin filaments in the stretched fibre, because they will then be closer together; or some change in the nature of the actin-myosin interaction when the filaments are under tension.

sion detonated on Amchitka Island (which lies almost at the 40° discontinuity) on October 29, 1965. As others have previously reported, there is a significant difference in travel times (after removing distance effects) between stations located on shield areas, on the one hand, and orogenic belts, on the other. But what Abe has now spotted is that travel times for stations in the azimuth range 10°–30° are significantly longer than those for stations in adjacent azimuthal ranges, even though the former are all shield stations. These differences are emphasized even more following station corrections and, in numerical terms, are estimated to be in the range 1 to 2 seconds.

On the broad scale, the azimuthal variation in the Longshot source term has been interpreted by Cleary (*Earth Planet Sci. Lett.*, **3**, 29; 1967) in terms of a thick, high-velocity (lithospheric) slab dipping steeply downward towards the north. Thus in general, earthquake-mechanism solutions indicate extensional stress more or less in the direction of the spreading lithosphere. In the region corresponding to azimuth range 10°–30°, however, the extensional stress is probably perpendicular to spreading motion; and the azimuthal variation in the source term indicates the presence of a structural anomaly which is most simply interpreted as a limited low-velocity region within the high-velocity slab. The P-wave velocity contrast between the two structures is 0.3–0.6 km s⁻¹.

Combining the two strands of interpretation, Abe suggests that at the 40° discontinuity the descending lithosphere tears and that asthenospheric material rises in the gap thus created to produce a local low-velocity region. This would form some sort of natural boundary which would explain why volcanism, geological structures and earthquake aftershocks very often appear to be discontinuous across the dividing line between the Western and Central Aleutians. Mogi (*Bull. Earthq. Res. Inst.*, **47**, 429; 1969), for example, has shown that whereas the aftershock epicentres of the Rat Island earthquake of 1965 were distributed in the Western Aleutians, those of the Andreanoff and Fox Islands earthquake of 1957 were limited to the Central Aleutians. In each case, the respective aftershock areas ended rather abruptly at 180° longitude, thus implying the presence of some natural barrier which effectively decouples one side from the other.

Finally, Abe's model offers a new insight into the nature of the Bowers Ridge, a totally submerged ridge which strikes northward from the Aleutian arc at the 40° discontinuity but which bends towards the west in a way which resembles, in both shape and direction, the left-hand Prince of Wales's feather,

On the basis of seismic reflexion and refraction observations, Ewing *et al.* (*J. Geophys. Res.*, **70**, 4593; 1965) and Ludwig *et al.* (*J. Geophys. Res.*, **76**, 6350; 1971) have interpreted Bowers Ridge as a thick, raised block of material comprising intrusive and extrusive rocks, initially formed probably in the middle Miocene (15–20 million years ago).

Abe now sees the Ridge in terms of an asthenospheric intrusion made possible by lithospheric tearing. If this view is valid, the Ridge would presumably have begun to form some time after the Pacific plate began to underthrust, which certainly means no earlier than the early Miocene (20–25 million years ago). In short, the two interpretations are similar except that Abe is able to relate his version much more closely to plate-tectonic processes.

ZOOLOGY

The Yearbook Again

IN the wake of the recent conference in Jersey on the breeding of endangered animal species as an aid to their survival (see *Nature*, **237**, 133; 1972), the special emphasis given to South American primates in this year's *International Zoo Yearbook* (Vol. 12, Zoological Society of London, £8.00) is most appropriate.

While there has rightly been concern recently over the status of certain rare apes and monkeys, the export of less endangered species from South America for research purposes, the pet trade and zoos has been continuing almost unchecked; some 35,000 primates are exported each year from Iquitos in Peru



Red uakari with infant born in Monkey Jungle Zoo, Miami (from the *International Zoo Yearbook*, Vol. 12).

alone, and populations of some species seem to be declining at an alarming rate. Several contributors to the yearbook thus urge more restrictions on the trapping and export of South American monkeys and there are useful notes on some breeding programmes and on aspects of primate biology.

The yearbook also includes its usual record of developments in animal husbandry, zoo architecture, veterinary research and so forth, not forgetting its useful reference section. Among the breeding successes noted is a red uakari (*Cacajao calvus rubicundus*), shown here, whose father was bred at the Monkey Jungle Zoo, Miami. This primate is listed in the *Red Data Book* published by the International Union for Conservation of Nature and is one of the few monkeys protected in South America.

Chromosome Fusion and DNA Loss

THE recent discovery of a new species of mouse, *Mus poschiavinus*, in a Swiss canton has provided Comings and Avelino, as they report in *Nature New Biology* next Wednesday (June 14), with an opportunity to measure the amount of DNA which is lost when V-shaped telocentric chromosomes fuse in pairs to form X-shaped metacentric chromosomes. Such chromosome fusions are common events in cultivated cells; they are also important in the evolution of new species and are presumed to result from cross-overs between DNA in the centromeric regions of pairs of telocentric chromosomes.

The karyotype of *Mus poschiavinus* includes seven metacentric chromosomes which seem to have arisen during speciation by the pairwise fusion of fourteen telocentric chromosomes in the karyotype of *Mus musculus*, the ancestral species. Mouse chromosomes contain large amounts of non-coding satellite DNA which has a simple

repetitive base sequence, a different buoyant density to the coding DNA and is concentrated in the centromeric regions. If therefore the fusion of the fourteen telocentric chromosomes was accompanied by the loss of considerable amounts (5–10 per cent) of the satellite DNA from the centromeric regions the total amount of satellite DNA in *Mus poschiavinus* cells should be significantly less than that in *Mus musculus* cells.

As Comings and Avelino report, however, analytical centrifugation of the DNA from the cells of these two closely related species of mice failed to reveal any significant difference in the amount of satellite DNA present. It seems that the maximum amount of DNA that can have been lost from the seven fusion events during the speciation of *Mus poschiavinus* is less than 0.5 per cent of the total genome; in other words, after each fusion at most less than 1.5×10^7 base pairs are eliminated.

Harvard's Hundred Years of Botany

by our Botany Correspondent

During the last week of May, Harvard University celebrated the centenary of the Arnold Arboretum, named after James Arnold whose bequest led to the garden's establishment in 1872.

AN international gathering of botanists and a hard working band of New England horticulture enthusiasts joined forces in Boston from May 21 to 28 to celebrate the centenary of Harvard University's Arnold Arboretum. Named after James Arnold, a rich Massachusetts merchant whose bequest led to its foundation, the arboretum is now a centre for many types of botanical endeavour, with more than 7,000 different trees and shrubs, about 900,000 herbarium specimens and a library of 78,000 volumes. But the week of tours and talks was also an opportunity for all Harvard's botanists to show off their treasures and achievements and it was not only the famous lilac blossom that attracted attention.

Botany at Harvard was dominated by Asa Gray, famous expert on the flora of North America, Fisher Professor of Natural History and head of the now vanished Botanic Gardens, when Arnold made his bequest in 1869. He left in trust to three colleagues part of his estate "to be by them applied for the promotion of Agricultural or Horticultural improvements, or other Philosophical, or Philanthropic purposes at their discretion". In spite of Gray's wish to add a tree collection to his botanic gardens, a piece of land some distance away at Jamaica Plain was provided by the university. Here the Arnold Arboretum was established on March 29, 1872. In that year Charles Sprague Sargent (Fig. 1), who had been an inauspicious undergraduate, was appointed director and began a distinguished reign that lasted until his death in 1927.

The early development of the arboretum was hastened by Sargent's proposal that the City of Boston should accept title to the land and lease it back to the university for a nominal sum. Thus in 1872 the arboretum became part of the Boston park system, which was laid out by the landscape architect Frederick Law Olmsted. The university paid—as it still does—a dollar a year, furnished the arboretum staff and supervised the planting. The City of Boston took over, with the ownership of the land, responsibility for building and maintaining roads, water supply and policing the ground. The final part of this bargain between town and gown was the free use of the arboretum as a public park from sunrise to sunset every day.

During Sargent's administration the collections of the arboretum grew steadily: dipping into his own substantial pocket, he supplied much of a herbarium of dried specimens of native woody plants, and started a botanical and horticultural library. He was also a great publicist, initiating the tradition of financial support for the arboretum from the wealthier members of the local populace.

According to the original agreement between the Arnold trustees and the university, the arboretum was to contain "as far as is practicable, all the trees, shrubs, and herbaceous plants, either indigenous or exotic" which could be grown at Jamaica Plain. But budgetary restrictions at first limited the living collection to North American and European species. Sargent, however, spread his net with an expedition to Japan in 1892, from whence he returned with several plants new to America. Several forays to the Orient followed, culminating in the four excursions of E. H. Wilson, a young Englishman whom Sargent hired in 1907 as plant

hunter. Funds were provided by the ever faithful patrons of the arboretum.

Wilson sustained many hardships in the wildernesses of western China, including a badly mangled leg which kept him sedentary in Boston for several years, but his rewards were many and glorious. He collected seeds of more than 1,000 species of trees and shrubs, and cuttings of willows, poplars and other trees. As well as flourishing in the arboretum, under the care of propagators William Dawson and William Judd, these plants also found their way into private gardens in the United States and Europe. Now that relations with China are improving again, American botanists are hopeful that they will be able to reopen their lines of communication and exchange information and material.

Meanwhile, as well as tending the living material sent back by Wilson and later travellers, the staff of the arboretum was spreading its influence through a series of books, including Sargent's own fourteen volumes of *Silva of North America* and the *Journal of the Arnold Arboretum*. When Sargent died in 1927 he had seen the arboretum grow from 125 to 250 acres. It had the greatest collection of hardy trees and shrubs in the northern hemisphere (Fig. 2), and the world's most important herbarium devoted exclusively to preserving the records of trees and shrubs.



Fig. 1 Charles Sprague Sargent (left) accompanied in 1880 on a collecting expedition to the west coast of the United States by Francis Skinner and George Engelmann. (Photo: Arnold Arboretum.)

A short era of reorganization was ushered in by the next director, Oakes Ames, who was also appointed director of the Botanical Museum and the Atkins Garden in Cuba, two separately endowed Harvard institutions. During this period, research at the arboretum diversified and, as well as taxonomy, plant pathology, genetics and wood anatomy were pursued, under the guidance of J. H. Faull, Karl Sax and I. W. Bailey respectively.

When Ames resigned in 1935, apparently for no other reason than to devote his energies to research and the Botanical Museum, he was replaced by E. D. Merrill, a taxonomist who specialized in the botany of South-East Asia.



Fig. 2 *Cephalanthus occidentalis pubescens* in the arboretum. (Photo: Arnold Arboretum.)

His years of office were marked by considerable hardship. First, there was the hurricane of September 1938 which destroyed approximately 1,500 trees, and then came the depression and the Second World War. The staff of the arboretum were, however, able to contribute to the war effort; Merrill prepared an army manual on the poisonous and potential food plants of the Pacific islands and some of his colleagues acted as consultants on camouflage problems.

Happier events during these hard times included the gift of the Larz Anderson collection of twenty-four dwarf Japanese trees, given by the widow of the late ambassador to Japan, and the acquisition of the Case Estates. In 1942 Miss Louisa Case endowed the arboretum with fifty-nine acres of land and buildings in Weston, Massachusetts, plus \$50,000. Shortly afterwards her sister, Marian Roby Case, left \$150,000 and a further ninety acres. This land provided much needed space for experimental cultivation.

After Merrill retired in 1946 and was replaced by Karl Sax, plans were set in motion for a new building to house the libraries and herbaria of several of Harvard's scattered botanical institutions. In 1954, when the present director, R. A. Howard, had taken over from Sax, the building was completed at the end of Divinity Avenue in nearby Cambridge. Part of the Arnold Arboretum's library and herbarium was moved in. The new order was opposed by some traditionalists, who made a long drawn out but finally unsuccessful attempt legally to restrain the university from instituting the changes. The forces of botanical innovation triumphed in 1966 when the Supreme Judicial Court of the State of Massachusetts handed down a verdict in favour of the President and Fellows of Harvard. Thus the legality of the move to Cambridge was confirmed.

The living and preserved collections of the arboretum now attract scholarly visitors from all parts of the world. The library has a full set of the works of Linnaeus and a fine series of books dating from the fifteenth and sixteenth centuries, thought to be the most complete outside the British Museum. Since the move to the university herbarium building, the arboretum's collection of wild plant material has been housed together with the Gray Herbarium, which was started in 1842 by Asa Gray, and is particularly rich in North American material. This nicely complements the Asian bias of the Arnold collection. The Oakes Ames Orchid Herbarium which occupies the same building is the largest such collection in the world devoted to a single plant family. Yet more treasures are to be found next door in the former divinity school library, which now houses the Farlow Library and Herbarium. These collections, devoted to algae, fungi, mosses and lichens, were endowed in 1919 by W. G. Farlow, a rich surgeon and botanist.

Botany at Harvard has not grown up within a single department, but under the auspices of several separately endowed institutions with their own directors, who manage

to coexist amicably, cooperating with the Department of Biology in the organization of teaching activities. The complex on Divinity Avenue is completed by the Botanical Museum, founded in 1888, under George Lincoln Goodale, to house Asa Gray's collection of "vegetable products, etc". Today the museum is a centre of research in economic botany and ethnobotany—the study of plants in archaeological remains.

The greatest treasure of the Botanical Museum, and the biggest attraction in all Harvard, however, is the renowned collection of perfectly reproduced glass flowers. This exhibit of about 3,000 items, including flowers, fruit and pollen of many species (Fig. 3), was made between 1887 and 1936 by Leopold Blaschka and his son Rudolph in their studio near Dresden, Germany. They were commissioned by Goodale, who wanted a display of the plant kingdom to parallel the exhibit of stuffed animals and skeletons in the Museum of Comparative Zoology. The glass flowers, which are sufficiently accurate to satisfy the demands of teachers today, were given to the Botanical Museum by Mrs Elizabeth C. Ware and her daughter Miss Mary Lee Ware in memory of Charles Eliot Ware of the Harvard class of 1834.

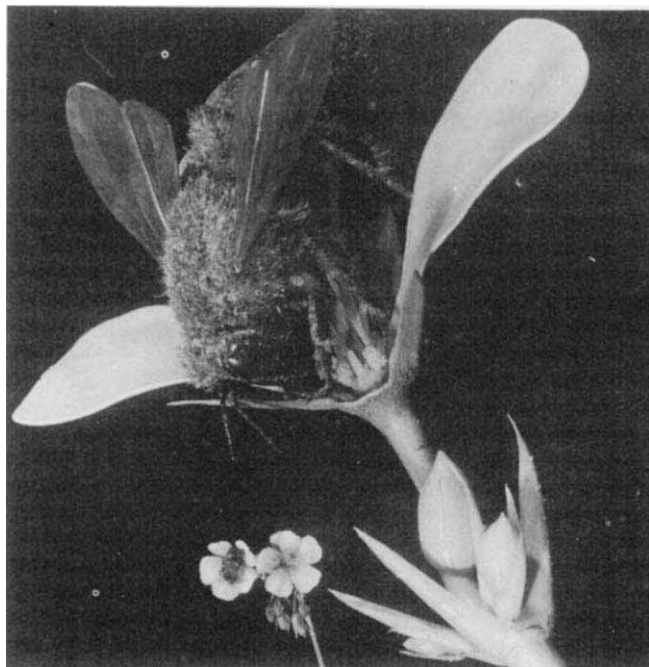


Fig. 3 One of the Blaschka glass models in the Botanical Museum at Harvard. The flower is *Oxalis valdiviensis* and the insect is a female *Halictus calceatus*.

It was inevitable that a unique collection of this sort would steal some of the limelight from the Arnold Arboretum during the week of its centenary celebrations. But it was trees, and above all the aura of the man who established them, that dominated the proceedings. At the centenary banquet on May 21, Dr William T. Stearn, guest of honour from the British Museum (Natural History), described Charles Sprague Sargent as an Old England landed gentleman in a New England setting. He was born into a wealthy society that was becoming increasingly interested in the planting and maintenance of trees to replenish and enrich the landscape. Stearn likened Sargent to such members of the English aristocracy as the Sixth Duke of Bedford (1766–1839), who developed the arboretum at Woburn Abbey and sponsored botanical work by his gardener James Forbes. It was Sargent's autocratic rule that won for the Arnold Arboretum a unique place among botanical institutions, which the inhabitants of Boston and Cambridge, and the botanists of the world still appreciate.

Cambridge University Natural Science Club, 1872 to 1972

M. B. PEPYS

Trinity College, Cambridge

1972 marks the centenary of the Cambridge University Natural Science Club, the foundation of which was reported in *Nature* on June 6, 1872, with an account of the first terminal report of the club. The contributions of past members to science have been considerable and far exceed what might be expected in view of the method of election and the restricted total membership.

THE Cambridge University Natural Science Club was formed on March 10, 1872, by a group of six undergraduates and BAs of Cambridge University for the "cultivation of natural science by means of friendly intercourse and mutual instruction" (original rule 1), and it has flourished continuously ever since, apart from the period 1914 to 1919. The motto of the club is "Omnia quae secundum Naturam fiunt sunt habenda in bonis" (Cicero)—"Works of Nature must all be counted good." The number of ordinary members has always been limited by the rules so that the meetings, held weekly during full term, could be held in members' rooms. According to the rules, the host for the evening at each meeting "brings some subject of scientific interest under the consideration of the members in the form of a short paper with such practical illustration as the subject admits of". It is recorded in the minutes of the first meeting in 1872 that: "A special clause has been inserted into the rules to guard against that degeneration of the meetings into merely social gatherings which is so often the destruction of university clubs". This rule was: "That at the ordinary meetings the refreshments be limited to tea and coffee", and, although conformity with the letter of this law did not last long, its spirit has been continuously upheld.

Membership of the Club

The average number of ordinary members at any particular time has usually been about ten to twelve, and it has never exceeded twenty-two. Candidates for election, drawn from undergraduate or postgraduate members of the university, have always been introduced initially as guests of current members of the club at several meetings before their formal proposal, and since 1942 they have also been required to read a paper lasting five minutes on a subject of their own choice. Election has been by secret ballot in the absence of the candidate, with success dependent on a three to one majority in favour. Ordinary members have in general retired on going out of residence or else at about the time of taking their MA. Past members who have achieved considerable scientific distinction, and sometimes

other distinguished scientists resident in Cambridge, have been elected to honorary membership with the obligation to read a paper once every few years.

Achievements of Past Members

As early as the five hundredth meeting of the club in 1894—an event marked by a very successful *conversazione* held in the Physiological Laboratory and attended by about 600 persons, "including a considerable number of the most eminent men of science, both British and Continental"—the past members had attained some distinction. The report of this auspicious occasion in *The Times* notes that "although the aggregate number of members in the club during these twenty-two years (since its foundation) only slightly exceeds 150, it may be observed that this number includes sixteen Fellows of the Royal Society and eighteen professors holding chairs in British and Colonial universities". In spite of a lapse of five years caused by the First World War, Sir J. J. Thomson, speaking as a guest at the dinner celebrating the thousandth meeting in 1920, was able to say: "This Club has played an important part in the history of science. Looking through a list of Fellows of the Royal Society I have been amazed to find that over seventy had been members of the Cambridge University Natural Science Club. The percentage is probably higher than that of fellows of colleges who have subsequently become FRS". Among those past members who had distinguished themselves at that time were two Nobel laureates, E. Rutherford (1908) and W. L. Bragg (1915).

In these early years of its existence the achievements of members could well be attributed to the predominance of the Cambridge School of Natural Science as a whole, resulting both from the financial and other resources of the university and its long tradition stretching back to Newton. The record of success has, however, continued unabated so that in 100 years the membership of the club has made a unique contribution to British science and indeed to scientific knowledge in general. At present the total of the past and current membership, excluding honorary members who were not previously ordinary members, is 601. Of these, 511 have been involved in the pursuit of academic careers or the practice of one of the scientific professions; 240 have undertaken some form of scientific research, and 119 of these have been elected to the Royal Society. In fact, thirty-seven (4.94%) of the present total Fellowship of the Royal Society (745) are past members of the club. Five past members have been Presidents of the Royal Society—E. Rutherford, H. H. Dale, E. D. Adrian, P. M. S. Blackett and the present incumbent A. L. Hodgkin, all of whom won Nobel Prizes as have five other past members: W. L. Bragg, G. P. Thomson, R. L. M. Synge, M. H. F. Wilkins and A. F. Huxley. These ten constitute 20% of all the British Nobel Laureates for science. Among the many scientific and other distinctions which the past members have achieved, the Order of Merit has been received by four: Rutherford, Dale, Adrian and G. I. Taylor.

Club Minute Books

The minute books are a fascinating record of all the meetings of the club since its foundation, and they also contain extensive correspondence between past and present members at different times. A remarkable constancy in the nature of the meetings and of the club's customs and activities emerges from the record, in spite of not infrequent, but usually minor, alterations to the rules. Even major changes enforced by the prevailing conditions in the university and the country as a whole, such as war, or innovations, such as the admission of women in 1942, seem to have had no significant effects on either the club or the subsequent achievements of its members.

In view of their limited numbers the uninterrupted contribution to original scientific thought over the course of a century of those members who pursued research careers merits consideration. It clearly reflects the sound judgment by members of their contemporaries, and their ability regularly to elect new members to uphold the club's traditions. A further factor which emerges from the correspondence of many members is the stimulation and information they received from the diverse interests of their colleagues. For example, W. A. H. Rushton, on retiring from membership in 1928, wrote: "It is my belief that my 'speech centre' is situated in my right cerebral hemisphere, and that my 'writing centre' is in my left, in any case I read slowly and with difficulty. And it thus comes about that nearly all the general scientific knowledge I possess, I have derived by the pleasant process of hearing it discussed by those who are interested in knowledge for itself. But even in my own subject I owe a great deal to the Club. Physiological research abounds in diverse problems—the intimate nature of polarization, how to crack a crayfish to get at its nerve, the solution of differential equations, how to keep algae from sprouting in the Ringer's fluid—and for each of these the Science Club provides an expert who is willing to give advice and help". In 1948 another retiring member wrote as follows: "It is some years now since I enjoyed the first of a long series of papers and discussions, on whatever evening was at that time deemed to be Friday, and I can most sincerely say that every one since has proved the extreme versatility of mind of the members, which is, after all, the most attractive feature of the Club. I hope it may prosper in the future, and that it may continue to find ways and means of illuminating one scientific discipline by the light of others until the real meaning of the gene, or entropy or something or other, is revealed—preferably to a biochemist, for they have waited long enough for some sort of revelation".

The experience gained by members who have to present papers to a friendly and yet unreservedly critical audience at an early stage in their scientific career has been considered extremely valuable by many past members. Lord Adrian, speaking at the dinner celebrating the 1,900th meeting in 1956, expressed himself on this point: "I have always been extremely grateful to the Club for electing me when I was still a freshman. I managed somehow to conceal my ignorance, though I still remember the nightmare of doing my first paper. But the Club swallowed it. . . ." On retiring from the club in 1923, F. J. W. Roughton wrote as follows: "I think specially of the Science Club, not only on account of the continuous inspiration which four years of Saturday evenings have given me but also because the preparation of my first paper before the Club led me straight into the field of research which has fascinated me so much ever since".

Papers Delivered

In spite of the profound changes which have taken place in scientific research, the papers delivered have continued to represent the active interests of the members in fields in which many of them have been destined to make important contributions. Members have been privileged to hear all of the ten Nobel Laureates talking about the subjects for work in

999th W. L. Bragg May 2nd/1915

The 999th meeting was held in Mr. Bragg's room, Trinity, the President in the chair. The reading minutes of the last meeting were read & after a slight alteration, were confirmed. . . . Mr. Bragg's paper was prepared by Mr. Montague & seconded by Mr. Brown. The President then called upon himself for his paper on "The Diffraction of X rays by Crystals."

The discovery by Laue that crystals were capable of diffracting a beam of X rays, has afforded a new method of analysing the structure of a crystal. When a narrow beam of X rays falls on a small slip of crystal, it is partly broken up into a series of diffracted beams, and if these are allowed to fall on a photographic plate they form a pretty pattern of spots obviously arranged in a geometrical manner. The effect may be thought of as a partial reflection of the X rays in the planes of the idea space lattice on which the crystal structure is built up. Simple crystals give very simple patterns, which can be considered as arising from a space lattice of points, one of the fourteen Bravais types. By tracing a particular space lattice one gets the simplest interpretation of the crystal pattern, it becomes easy to determine the fundamental arrangement of molecules, and even of atoms, inside the crystal. Examples of the analysis of a series of crystals such as Sodium chloride, Potassium chloride, Potassium Bromide, Potassium iodide etc. were given, and a number of diagrams were shown of photographs taken with various crystals.

After considerable discussion, the meeting was declared closed, there having been present

Mr. Adrian,
Thompson
Montague
Bragg
Brown.
Kennedy
King.
Denny
Evans
Clark.
J. P. Thomson.

W. L. Bragg President.

Fig. 1 W. L. Bragg's summary, in the club minute book, of one of his own papers.

which they subsequently were awarded the prize as well as about a range of other topics illustrative of the diversity of their interest. Between 1896 and 1898 Rutherford gave a series of papers on "A New Method of Detecting Electrical Oscillations" (by the effect they produce on magnets, and demonstrated his own apparatus), "Invisible Radiation" (dealing particularly with the emission of uranium and its compounds) and "Kathode Rays" in which "he discussed the extremely interesting speculations as to the ultimate constitution of matter". Dale, a contemporary of Rutherford, discoursed on "Internal Secretion" (1897) and "Electromotive Activity of Gland Cells" (1899) among other topics. In 1913 W. L. Bragg presented a paper on "The Diffraction of X-rays with Crystals" which is summarized in his own hand in the minute book (Fig. 1). His observations, which founded the science of X-ray crystallography and paved the way for the development of molecular biology, represent the work for which he received the Nobel Prize in 1915, gaining thereby



PHYSICAL LABORATORIES,
THE UNIVERSITY,
MANCHESTER.

Dec 21 1908

R H Crompton Esq
Hon sec CUNSC.
Dear Sir

I much
appreciate the kind
congratulations of
the CUNSC on the
award of the Nobel Prize.
I have a very pleasant
recollection of my connexion
with the Society during
my stay in Cambridge
Yours sincerely
Ernest Rutherford

Fig. 2 Reply from Ernest Rutherford to a letter of congratulation sent by the club on the occasion of the award of the Nobel Prize.

the unique distinction of doing so while still a member of the club. Among those present at that meeting in 1913 were two future Nobel Laureates, Adrian and G. P. Thomson. In fact between the late 1890s and 1940 there were only brief periods during which there was not at least one future Nobel Laureate among the members. Other examples of papers by these men in the fields to which they later contributed are as follows: "The Investigation of Nerve and Muscle" (1911) and "The Nervous Impulse" (1914) by E. D. Adrian; "Positive Rays" (1914) by G. P. Thomson; "The Reality of Atomic Models" (1921), "Alpha Particles" (1923) and "Spectra and the Periodic Table" (1924) by P. M. S. Blackett; "Membrane Theory of Nervous Action" (1934) and "Nerve-Muscle Functions" (1937) by A. L. Hodgkin; "Technique in Biochemistry" (1936) and "Protein Structure" (1936) by R. L. M. Synge; "Seeing Structures" (1938) by M. H. F. Wilkins; "The Conduction of Nervous Impulses" (1939) and "Experiments on Single Cells" (1940) by A. F. Huxley.

Correspondence of Past Members

The humility and humour of the feelings of past members towards the club is revealed in their many replies to letters of congratulation from the present membership on receipt of honours. Ernest Rutherford wrote in 1908 (Fig. 2): "I much appreciate the kind congratulations of the CUNSC on the award of the Nobel Prize. I have a very pleasant recollection of my connexion with the society during my stay in Cambridge". In 1910 Joseph Barcroft was elected to the Royal Society and replied to the club's congratulations: "Please

thank the N. Science Club for their good wishes and congratulations. It has always been an article in my creed that there was an even chance of any member of the Natural Science Club becoming a Fellow of the Royal Society if he stuck to science, though I need hardly say I never ventured to apply the theory to my own case. But I feel as sure now as I did when a student that no young man at Cambridge could have a greater distinction than to belong to the N.S. Club and I look back as a past president to very many happy Saturday evenings spent at its meetings". In the same year Edward Mellanby wrote as follows: "... convey my best thanks to the members of the Science Club for their kind congratulations on my appointment to a Beit Fellowship. May I utter a word of warning? If the Club extends its congratulations to all its old members attaining such dignities as Beit Fellowships (when they all get going), the lives of your worthy successors in the capacity of Honorary Secretary will, no doubt, be hastened to a condition of premature senility by the overwhelming clerical work. Probably no secretary would survive or be capable of assuming the Presidency and without a President the Club would collapse. That would be the end of all things."

On his election to the Royal Society in 1921, W. L. Bragg wrote: "I do not think that my pleasure was any greater than it was on the proud day when I was told that I had become a member of the CUNSC". Dale was invited to become an honorary member of the club in 1923 and replied: "There is no honour which I could more highly value and I gladly and gratefully accept." After the award of the Nobel Prize in 1937, G. P. Thomson wrote: "Will you convey to the CUNSC my sincerest thanks for their congratulations. I can now flatter myself that I have not entirely wasted the scientific opportunities which I enjoyed as a very faithful member of the Club. May it long flourish!" The club's congratulations to W. A. H. Rushton on his election to the Royal Society elicited the following reply: "A few weeks ago my wife reminded me that I had told her that one of my chief ambitions was to receive from the CUNSC a letter such as the one you have just sent me. It is hardly necessary to say more to indicate the satisfaction it gave me. I well remember when a member of the Club wondering, as no doubt the present members wonder, whether I should ever have this pleasure. I fortified myself with the fact that well over half of the present members who stayed in science had become FRS. And I expect the same still holds for you. In fact the CUNSC should receive a letter of thanks from the RS at so ably giving the lead in choice of suitable fellows." As a final example, in 1951 Adrian became President of the Royal Society and wrote to the club: "Since I owe my whole scientific career to the stimulus given by membership of the CU Natural Science Club it gives me the greatest pleasure to have the Club's congratulations. There can be no other Club which has fed the Royal Society so well: in fact it would be a national disaster if the Club ceased to exist or to keep up the high standard of its membership and the wide range of their interests. I am sure that all the past members would agree with me in thinking that their election was the greatest honour that happened to them in Cambridge and in feeling very proud if their subsequent career could be said to have justified it."

Two letters from two of its great men epitomize the influence they attributed to their membership of the club: Bragg in 1937, "My election to the Club was a great day in my life, my first scientific distinction," and Adrian in 1951, "I am sure the members will realize the apprehension with which I contemplate having to fill such an exalted post (Master of Trinity College, Cambridge). It does not seem so long since I attended the meetings of the Club and wondered whether I should ever be capable of scientific research, and I should certainly not have been without the stimulus of those meetings".

¹ *Nature*, 6, 110 (1872).

Bubble Raft Model for an Amorphous Alloy

A. W. SIMPSON & PAULINE H. HODKINSON*

Materials Science Division, School of Applied Sciences, University of Sussex, Brighton

A two-dimensional model of an amorphous alloy is given by a raft composed of two sizes of bubbles. The density, method of close packing and mechanical properties of the alloy are well simulated.

In the bubble raft model¹⁻⁴ of the crystal structure of metals, close packed hexagonal sheets of bubbles well represent grain boundaries, dislocations and other types of fault, the bubbles being held together by a general capillary attraction which represents the binding force of the free electrons in a metal. Bubble assemblages have the advantage over models involving the random close-packing of hard spheres⁵⁻⁹ in that two-dimensional models can readily be produced in large numbers. Arrays of up to 5,000 bubbles can be formed quite easily. A comparison of the pair distribution function (PDF) of a bubble mixture with that of Finney's hard sphere model¹⁰ gives some support for the validity of the bubble model. We report the application of the bubble model to amorphous alloys.

Preparation of the Bubble Rafts

We used a circular glass trough, roughly 20 cm in diameter and 10 cm deep, containing the soap solution¹. Air at constant pressure was driven through short capillaries, with 40 μm to 100 μm bores, to produce uniform bubbles with diameters of 1 to 2 mm, depending on the nozzle size, and independent of the depth of the jet and the pressure. The bubbles were photographed by transmitted light despite the opacity of the solution.

Rafts about 10 cm across were used containing between 2,000 and 5,000 bubbles; amorphous arrays (Fig. 1) were made by forming two regular rafts and mixing them slowly and thoroughly with a glass rod. To obtain suitable structures a minimum size difference of 12-13% was necessary. Bubbles of about 1 mm diameter show the best analogies with real metals³.

Mader *et al.*¹¹ propose that an amorphous structure is formed when close-packed components with a size difference greater than 10% are mixed in roughly equal proportions; components differing in size by more than 25% may still make a regular structure, the solute forming a crystalline array with the solvent occupying interstitial sites. By analogy amorphous structures should be produced from roughly equal quantities of bubbles differing by about 10 to 25% in size, in the 1 to 2 mm diameter range.

The bubble diameter was determined by measuring the closest distance between bubble centres of the uniform rafts

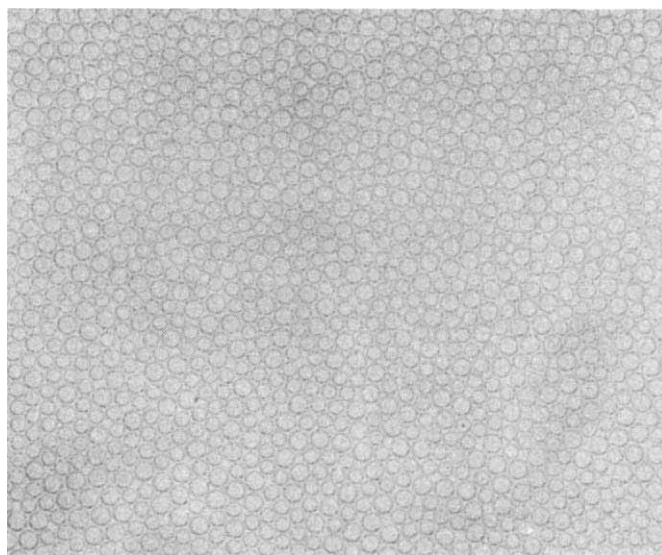


Fig. 1 Two-dimensional amorphous array of 1.19 mm and 1.43 mm diameter bubbles with roughly 50% solute. This is an 18% bubble size difference with respect to the mean diameter.

before they were mixed. We found that the difference in height of the solvent and solute bubbles in the mixed raft above the surface of the soap solution was negligible, provided that the size difference was less than about 30%.

The presence of collineations in Fig. 1 is in keeping with Bernal's model, where as many as eight spheres formed in a straight line.

Two-dimensional Pair Distribution Functions

Pair distribution functions for a hundred bubble centres in roughly 50/50 mixtures were calculated by measuring the coordinates of the centres of these hundred bubbles and of some 600 surrounding ones. Fig. 2 shows the results for a polycrystalline array and for two typical amorphous arrays with different bubble size ratios. Polycrystalline mixtures are obtained by insufficient mixing of solvent and solute. Small crystalline regions with roughly 20 to 30 bubbles in each grain are clearly visible on preparation.

In Table 1 the PDF maxima are compared with a 50/50 ordered two-dimensional hexagonal structure, and also with those obtained experimentally by Finney for the random packing of hard spheres¹⁰. All first coordination numbers deduced from Fig. 2 are practically equal to 6, which is to be expected in a close-packed two-dimensional structure.

* Present address: Department of Metallurgy, University of British Columbia.

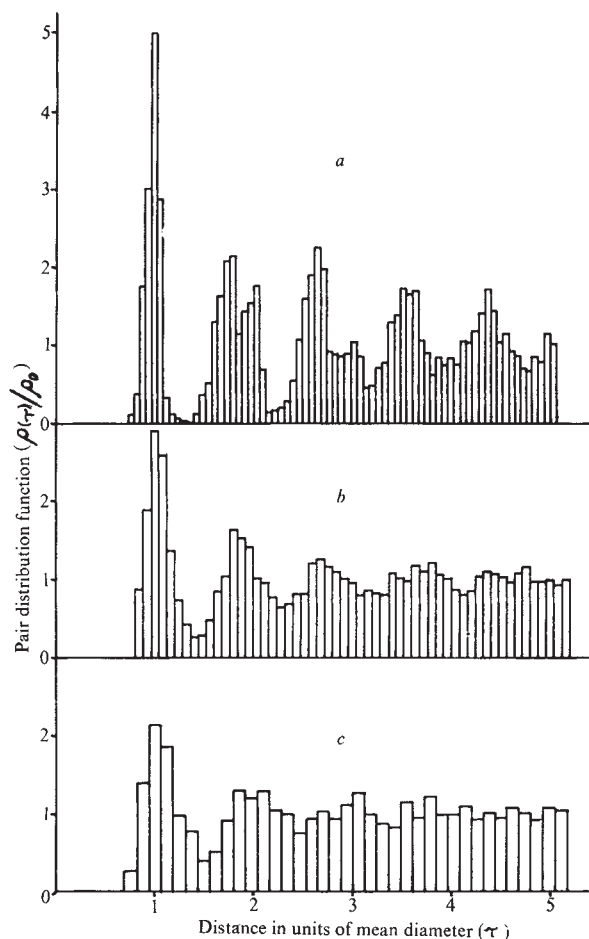


Fig. 2 Typical pair distribution functions for bubble rafts with roughly equal concentrations of solvent and solute bubbles. *a*, Polycrystalline array with bubble diameters differing by 20%; *b* and *c*, amorphous array with bubble sizes differing by 27% and 31% respectively.

The PDF for the amorphous arrays appears much more diffuse, falling off rapidly at larger r , reflecting the short range order of an amorphous structure. Table 1 shows some distinct differences between the experimental polycrystalline and amorphous PDFs. We found the better agreement of Finney's model with polycrystalline results rather surprising.

Table 1 Comparison of Pair Distribution Peak Positions

Coordination circle	Position of peaks in units of mean diameter				
	Ordered AB two-dimensional hexagonal array	Polycrystalline bubble raft (20% size difference)	Amorphous bubble rafts 27% size difference	Amorphous bubble rafts 31% size difference	Finney's hard-sphere three-dimensional model
1	1.00	1.00	1.02	1.02	1.00
2	1.73	1.77	1.88	1.87	1.73
3	2.00	2.01	2.04	2.09	1.99
4	2.65	2.65	2.80	2.69	2.65
5	3.00	3.00		3.07	
6	3.46	3.49	3.50	3.54	3.47
7	3.61	3.61	3.73	3.77	
8	4.00	3.85) 3.97)	3.88	4.15	
9	4.36	4.41	4.48	4.37	4.25
10	4.58	4.58		4.63	
11	5.00	4.88) 5.12)	4.82	5.00	

Vegard's Law

Other workers have attempted to obtain a criterion for amorphous structure formation in terms of deviations from

a relationship corresponding to Vegard's Law^{12,13}. Experiments were made with bubble rafts having various concentrations of solvent and solute bubbles, and a plot analogous to a "volume Vegard's Law" obtained. Fig. 3 shows the "average lattice area" or "reciprocal atomic density" plotted against the concentration of solute. For the limited available range of solute the data obey Vegard's Law. Using the bubble model we could not obtain an amorphous array for low or high concentrations and therefore the points corres-

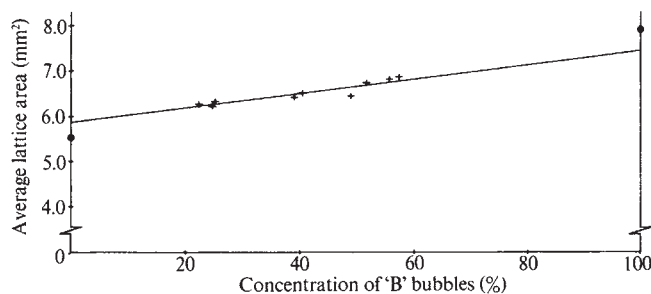


Fig. 3 The average lattice area as a function of concentration of solute bubbles for amorphous bubble arrays with a size difference of roughly 20%. +, Experimental points for the bubble arrays; O, estimated points as discussed in the text.

ponding to these values could not be obtained. We approximated by calculating the average lattice area for a regular array of bubbles of the relevant size and correcting for amorphousness, by finding the average lattice area for a close-packed amorphous array of equal-sized metal discs. The average lattice area for amorphous arrays differed by 4.8% from that of a regular hexagonal array of discs. The end points thus estimated are shown in Fig. 3, but agree only moderately well with extrapolation of the "least square" straight line.

Mechanical Properties

A sheet of regular sized bubbles is elastically isotropic and therefore two elastic constants are adequate to specify its elastic properties. Bragg and Nye found Young's modulus and Poisson's ratio to be the most convenient constants and therefore we measured these for the amorphous bubble array. The simple apparatus consisted of a collar resting on the top of the trough with portions removed to accommodate a nylon pulley and a deformation apparatus made from balsa wood sealed with varnish. Bubble rafts were formed between two smooth barriers, one fixed and one floating, and deformation occurred when small weights were placed on a scale pan attached to the moving barrier. Photographs of the central area of the raft were taken with differing weights and the elastic constants calculated from the longitudinal extension and lateral contraction (recoverable). The results for two different-sized regular bubble arrays and their amorphous mixture are shown in Table 2.

The Young's modulus measurements should be considered approximate as frictional errors occurred. The determina-

Table 2 Mechanical Properties of Rafts

	Young's modulus (kg/mm × 10 ⁻²)	Poisson's ratio
1.19 mm diameter regular array	1.9	0.25
1.43 mm diameter regular array	2.0	0.30
50/50 amorphous array	0.9	0.90

tion of Poisson's ratio, however, which is independent of the load, should be accurate.

Deformation of the amorphous array proceeded by the mutual adjustment of neighbours to more stable positions rather than by an extension and compression of the bubbles themselves. In fact the amorphous raft appeared to be much less rigid than either of the two regular arrays, this being confirmed by the smaller value of the Young's modulus. The significantly lower Young's modulus of the amorphous array compared with the crystalline forms of either of its constituents reflects the analogous situation in the real solid state where, for example, the Young's modulus of amorphous fused silica is roughly 35% lower than that for crystalline quartz. For glassy metal alloys several workers have reported similarly low values of Young's modulus¹⁴⁻¹⁶ with increases of 20-60% on crystallization. The bubble raft model also supports Orowan's¹⁷ suggestion that highly mobile regions exist within the matrix of glassy alloys which can sustain only negligible stress.

The high value of Poisson's ratio, associated with the low Young's modulus of the amorphous array, is not inconsistent since in a two-dimensional array the maximum theoretical Poisson's ratio is unity, not 0.5 as in three-dimensional systems. Cottrell¹⁸ points out that as Poisson's ratio approaches the maximum possible value materials become steadily more fluid; a Poisson's ratio of 0.9 therefore corresponds to a material of high fluidity and low rigidity.

The PDF for a well mixed bubble array showed similar features to those obtained on typical amorphous materials, showing a few broad peaks with their positions in reasonably good agreement with Finney's distribution function for his hard sphere model. A straight-line relation between average lattice area and solute concentration was produced for bubble mixtures with diameters differing by roughly 20%, agreeing with the work of Maitrepierre. The Young's modulus and especially the Poisson's ratio of the bubble mixture showed striking deviations from the regular arrays, as is also observed in analogous glassy alloys.

Thus it appears from this brief study that the bubble raft

can be used as a good model for an amorphous alloy provided Mader's criteria are satisfied. The capillary attraction of the bubbles ensures dense packing and good mixing produces random arrays. Some bubbles tended to align but, as Bernal stated, there will always be a certain degree of collineation in a liquid or amorphous array.

Further study of the polygonal holes present is suggested to obtain a Bernal model for a two-dimensional amorphous structure. This might be compared with the prediction of the three-dimensional amorphous models put forward by Polk⁸ and other authors in terms of the morphology of the cavities.

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How *lac* Repressor Binds to DNA

K. ADLER, K. BEYREUTHER, E. FANNING,
N. GEISLER, B. GRONENBORN, A. KLEMM*,
B. MÜLLER-HILL, M. PFAHL & A. SCHMITZ

Institute für Genetik, Universität zu Köln, 5 Köln-Lindenthal, Weyertal 121, W. Germany

A sequence of about fifty amino-acids at the N-terminus of the *lac* repressor is thought to bind directly to *lac* operator DNA. Although this length cannot make the conventional cleft, it could form a protrusion; the manner in which such a protrusion interacts with the DNA site is discussed here.

THE binding of *lac* repressor to *lac* operator is a good example of a specific and tight interaction between protein and DNA. *Lac* repressor binds only to one (or very few) sites on the *E. coli* chromosome with the remarkable dissociation constant of 10^{-13} M^{1,2}. *Lac* repressor has to recognize at least twelve base pairs, if we assume the *E. coli* chromosome to be a random polymer¹. Little is known about the structure of *lac* operator, and not much more about *lac* repressor, which is a protein (MW 160,000) composed of four identical subunits³, and careful work has shown that pure and active repressor does not contain any DNA or RNA³. The interaction between DNA and *lac* repressor is therefore presumably a direct interaction between protein and DNA.

We predict that *lac* repressor, unlike a typical enzyme, binds to *lac* operator with a protrusion rather than a

*Deceased March 13, 1972

cleft: this prediction is based on genetic and chemical data. We have isolated a large number of mutants in which *lac* repressor has lost the capacity to bind *lac* operator, but not *lac* inducer. In such mutants we expect the site which makes contact with the DNA to be predominantly affected. We found that all such mutants map in the region of the *i* gene, which codes for the amino-terminus of *lac* repressor sequenced by us.

An indiscriminate analysis of all mutants which lack a function is useless as too many mutations will wreck the whole structure of the protein and not just the binding site. One has to look for mutants in which only the binding site is destroyed, but in which the general three-dimensional structure of the protein is left intact. They should exist as, by mutation, every amino-acid can be exchanged for many other amino-acids.

Lac repressor is suited to such an analysis since it has two active sites: one binds operator, the other binds inducer. Mutants which have lost the capacity to bind inducer but not operator were described as early as 1964⁴. Mutants whose capacity to bind operator is lost but whose capacity to bind inducer is retained had not been found when we started this work, although there existed a class of candidates, the transdominant constitutives (*i*^{-d}). These mutants map in *i*⁵, are transdominant⁶ and bind *in vitro* inducer⁷. The transdominance had been explained as being due to the capacity to form either inactive mixed oligomers with wild type repressor, or inactive

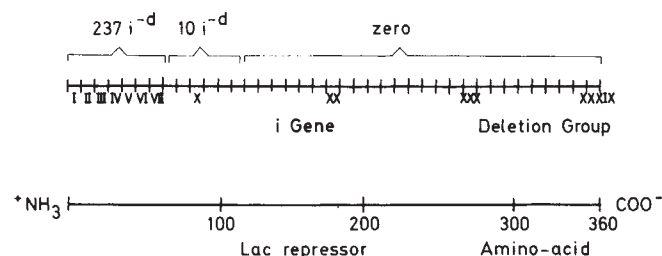


Fig. 1 Crude map of 247 *i*^{-d}s. The abscissa represents the *i* gene. It is divided into 39 deletion groups. We have given equal space to each deletion group but this does not imply that the physical distances between deletion groups are equal. That most of the deletions end at different points on the map is proved by the fact that most of them could be separated in crosses against various *i* gene mutants⁹. The promoter of the *i* gene (*q*) maps at the outer left end, the *lac* operator at the outer right end of the map⁸. The left end of the *i* gene codes for the amino-terminal end of *lac* repressor. Most mutants were isolated after treatment of the parent strain with nitrosoguanidine. Details of the procedure used for selection¹⁰ and mapping will be published elsewhere (Pfafl and Miller, work in preparation).

operator complexes. We performed *in vitro* tests which excluded the latter. If transdominance is in fact due to the formation of mixed inactive oligomers, these mutants are perfect for our analysis. They have the capacity to aggregate and to bind inducer but not to bind operator.

As a result, we isolated and analysed 247 *i*^{-d} mutants. *In vitro* tests showed that 32 of 34 of the *i*^{-d} repressors bound inducer *in vitro*. 22 of 22 did not bind operator *in vitro* (Table 1). Crude mapping indicated that 237 *i*^{-d}s mapped between deletion groups I and VII of the *i* gene, which corresponded to the amino-terminus of *lac* repressor⁸. The remaining mutants mapped between deletion groups VIII and XIII (Fig. 1). The *i*^{-d}s which mapped in deletion groups II to VII were fully constitutive and strongly transdominant ("normal"). Most of the *i*^{-d}s which mapped in deletion groups VIII to XIII were superinducible and only weakly transdominant

Table 1 Properties of Two Typical *i*^{-d}s

Mutant (A)	Inducer binding (B)	Operator binding (C)	Trans-dominance (D)	Constitutivity (E)	(F)
<i>i</i> _{BG 1} ^{-d} normal	1.0	± 50 c.p.m.	300	600	600
<i>i</i> _{BG 124} ^{-d} abnormal	1.0	± 50 c.p.m.	50	300	600

A, The mutants (10) were derived like most of the 247 *i*^{-d}s from a strain which overproduced *lac* repressor (*i*^q) (ref. 11).

B, Specific activity of *lac* repressor as defined in ref. 3. The specific activity of less than half the "normal" *i*^{-d}s tested was less than 1. Only one mutant (BG 7) had a specific activity lower than 0.05. The specific activity of most "abnormal" *i*^{-d}s was around 1.0.

C, Filter test for operator binding as described by Riggs *et al.*². 20 µg protein was used per test. Total input of ³²P-labelled *λ*h80 *dlac*, DNA was 3,600 c.p.m. Extracts from the wild type (parent) strain retained 1,000 c.p.m. on the filter. In the presence of inducer this value dropped to ± 50 c.p.m.

D, Specific activity of β-galactosidase in a test for transdominance: the following strain was tested for β-galactosidase: [*lac pro*_{AB}]Δ *RecA SmR* φ80*dlac i*⁺*z*⁺*y*⁺/F'*lac i*^{-d}*z*⁻*u*₁₁₈ *pro*_{AB}. Test as described in ref. 8.

E, The following strain was tested for β-galactosidase after growth without inducer: [*lac pro*_{AB}]Δ *SmR F*'*lac i*^{-d}*z*⁺ *pro*_{AB}. Test as described in ref. 8.

F, The following strain was tested for β-galactosidase activity after growth in the presence of inducer (10⁻³ M IPTG): [*lac pro*_{AB}]Δ *SmR F*'*lac i*^{-d}*z*⁺ *pro*_{AB}. Test as described in ref. 8.

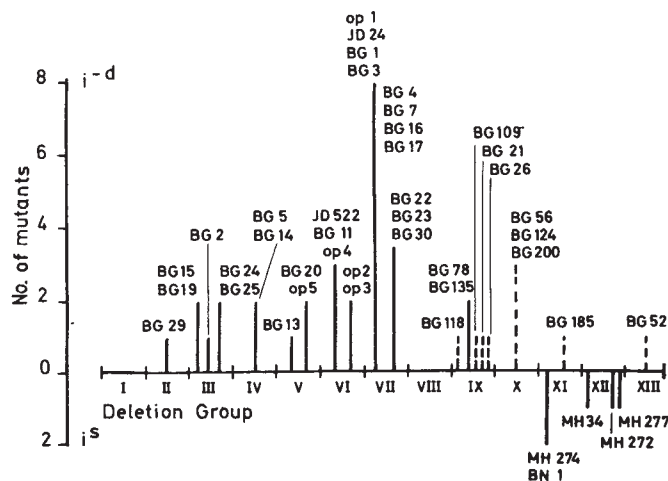


Fig. 2 Detailed map of some *i*^{-d}s and *i*^s. The abscissa represents the left part of the *i* gene (see Fig. 1). The ordinate gives the number of mutants which did not recombine with each other. Not all mutants in deletion group VII were crossed against each other¹⁰. Bars pointing upward represent fully constitutive *i*^{-d}s, broken bars pointing upward semiconstitutive *i*^{-d}s. Bars pointing downward represent *i*^s.

("abnormal") (Table 1). Fig. 2 indicates the map position of the mutants which we mapped accurately. We found at least 11 different sites for the *i*^{-d}s mapping in the deletion groups II to VIII.

We also isolated five mutants deficient in operator binding without relying on transdominance for selection, by screening 200 *i*^{-d} constitutives. These *i*^{-op}s were transdominant, bound

Table 2 Des-arg-des-lys-*-lac* Repressor Binds Inducer but not Operator

Material	Inducer binding (A)	Operator binding (B)
<i>lac</i> repressor untreated (C)	1,112	716 c.p.m.
<i>lac</i> repressor untreated (D)	1,159	630 c.p.m.
<i>lac</i> repressor + carboxypeptidase B: 0 min incubation (E)	949	651 c.p.m.
<i>lac</i> repressor + carboxypeptidase B: 95 min incubation (F)	1,032	7 c.p.m.

A, Specific activity of *lac* repressor as defined in ref. 3. The concentration of added carboxypeptidase B has been subtracted from the total protein concentration.

B, Filter test for operator binding as described by Riggs *et al.*² Total input of counts was 3,600 c.p.m. ³²P labelled λ h80 *dlac* DNA. The same amount of *lac* repressor (0.012 μ g) was used for all tests.

C, *Lac* repressor was tested without any pretreatment.

D, *Lac* repressor was incubated for 95 min at 37° C but without added carboxypeptidase B.

E, 0.5 mg *lac* repressor and 0.67 mg freshly dialysed carboxypeptidase B (Merck) were mixed in TMS buffer³. The tests for operator and inducer binding were done immediately at 4° C.

F, The mixture of *lac* repressor and carboxypeptidase B was incubated for 95 min at 37° C. Then it was chilled and aliquots were taken to test inducer and operator binding at 4° C. Some material was also analysed on an amino-acid analyser. Samples of carboxypeptidase B and *lac* repressor treated separately served as controls. We cannot exclude that impurities in the carboxypeptidase B lead to additional cuts in the repressor.

Thus we predict protein regions corresponding to deletion groups II to VII to be directly involved in binding. We know that a mutation which maps in deletion group VI correlates with a change in protein structure to the left of amino-acid No. 42¹⁵. We predict that some of the first 50 amino-acids form the site which binds to DNA. Fifty amino-acids alone cannot surround a cleft which specifically binds several bases. Although 50 amino-acids are not enough to form a cleft, they could certainly form a protrusion which binds to *lac* operator, if a protrusion is suited for such binding.

Crick¹⁶ argued recently that DNA (at least in higher organisms) must be single-stranded to be recognized by protein. He reasoned that all enzymes whose structure was known by X-ray crystallography had clefts but not protrusion as recognition elements. This is certainly true: but none of the enzymes whose structure is known binds to DNA specifically. The structure of DNA might, however, invite protein to use a protrusion as recognition element.

One could envisage the operator DNA to be denatured and the protrusion interacting with it in complicated and non-predictable ways. Such structures do not look attractive to us. If, however, we assume that DNA and protein are present in the most simple and stable structures (as double helix¹⁷ and as α helix¹⁸) some interesting consequences follow.

Sung and Dixon have shown by model building that an α helix fits in the deep groove of DNA in the B configuration¹⁹. They proposed that a particular histone bound non-specifically in this way to DNA, using its lysines and arginines to interact with the phosphate residues of the backbone. It is logical to postulate that an α helix might not only bind to the backbone of the DNA but also—with suitable side chains—specifically to the polar groups of the base pairs in the deep groove. An inspection of the deep groove shows that the four base pairs can be distinguished from each other (Fig. 4).

Playing with a space filling model showed that there is only one region between amino-acid 1 and 50 of *lac* repressor which can bind to the backbone as well as to the polar groups of the deep groove of DNA: it extends from amino-acid 17 to 33. Arginine 22, histidine 29 and lysine 33 can bind to phosphates of the backbone; tyrosine 17, glutamine 18, serine 21, aspartic acid 25 and glutamine 26 can potentially form hydrogen bonds with polar groups in the deep groove. It is reassuring that this region alone is the one for which one predicts the highest degree of helicity, if one analyses the amino-terminal sequence according to Schiffer and Edmundson²⁰.

We realized very soon that the finding by model building of a particular DNA sequence which binds this region better than all other DNA sequences depends very much on the general picture one has of this interaction. We pictured it thus: a large part of the binding energy comes from the electrostatic attraction between the negatively charged DNA and the positively charged groups of the *lac* repressor, which is suggested by the great dependence of the binding constant on salt concentration^{1,2}. Moreover, we reasoned that the hydrogen bonds formed between base pairs and the fitting amino-acids would not be much stronger than the hydrogen bonds which the separate molecules would form in water. Nonfitting base pairs would, however, repulse the peptide. Repulsion of the peptide would be particularly strong if not all polar groups in the deep groove could form hydrogen bonds to water or to side chains of the peptide. This assumption seems to exclude the use of N7 of the purines as electron donor to side chains, as the other polar groups in the interior of the groove seem to be unable to form hydrogen bonds to water in these conditions. Furthermore, while building the model we tried to avoid wrong bond angles, steric hindrance by the methyl group of thymine and equal charges in close distance

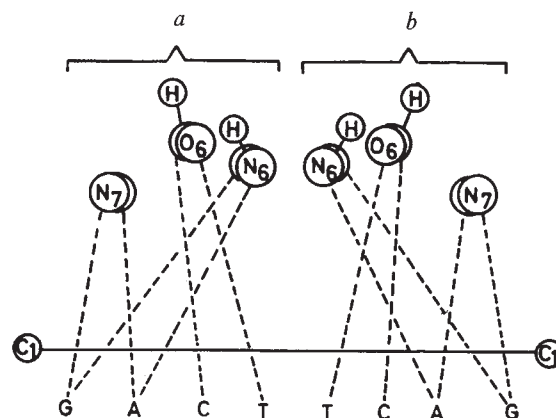


Fig. 4 The polar groups in the deep groove of DNA in the B configuration. The C1 atoms of the two deoxyriboses are the fixed points for the orientation of the four base pairs. The A/T pair is drawn above the G/C pair, the T/A pair above the C/G pair. The dotted lines leading from N(7), NH or O to an A, T, G or C indicate to which base the respective polar group belongs. The 3' end (a) of the left deoxyribose and the 5' end (b) of the right deoxyribose point in the direction of the viewer. The relative distances are taken from X-ray data.

(especially in the case of aspartic acid), and we tried finally not to distort either helix. There is one fact which might suggest that operator DNA is not grossly distorted when binding to *lac* repressor: the activation energy of formation of the complex is only 8.5 Cal²¹. The restriction of keeping both helices absolutely undistorted is arbitrary, no matter how necessary it may be for model building.

Fig. 5 Kendrew model of DNA interacting with the amino-terminus of *lac* repressor. The DNA is in the B configuration, the interacting peptide an alpha helix. The peptide lies in the deep groove of the DNA. The base pairs which form hydrogen bonds to the side chains of the peptide are numbered. Base pair I interacts with tyrosine 17, whose ring can be seen. Base pair II interacts with glutamine 18 (which can be seen) and serine 21. Base pair IV interacts with glutamine 26 (the hydrogen bond can be seen). Above base pair IV arginine 22 can be seen to interact with a phosphate. Near base pair VI lysine 33 can be seen to bind in the background with a phosphate of the same strand. Near base pair III histidine 29 can be seen to interact with a phosphate of the other strand.

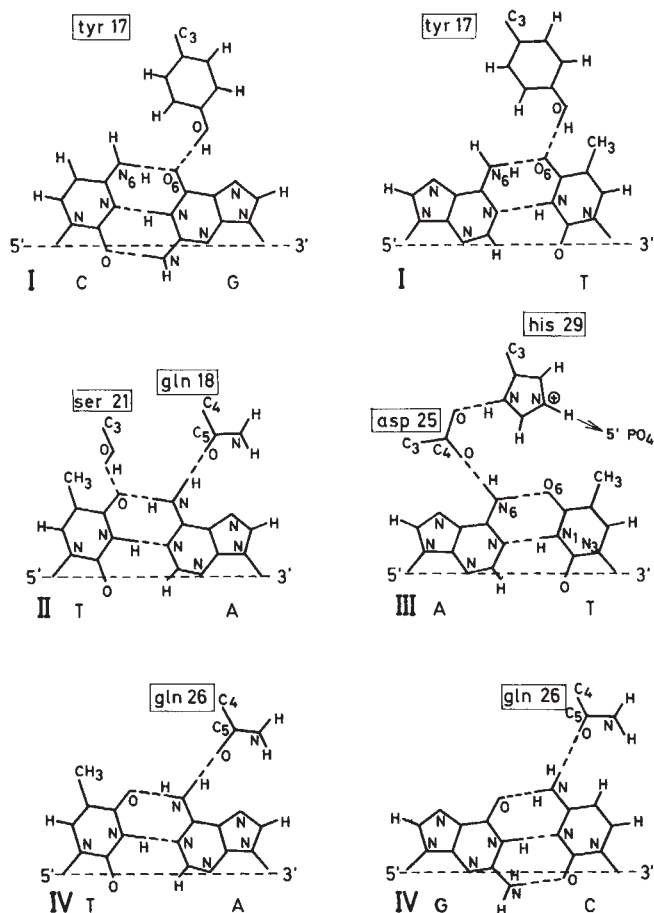
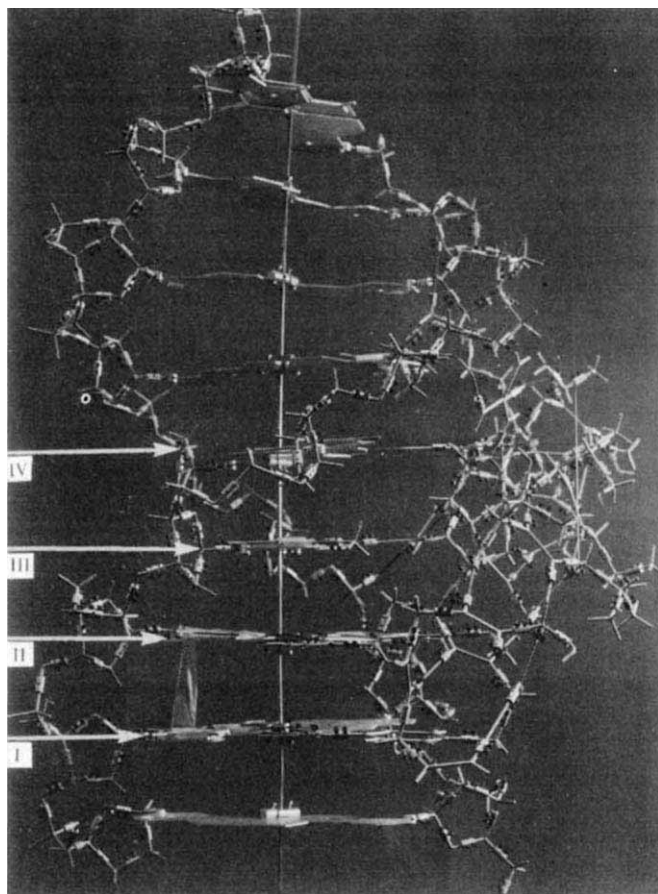


Fig. 6 Possible interactions between base pairs and side chains of the N-terminus of *lac* repressor. I, Interaction of tyrosine 17 with the first base pair: ${}^5\text{C/G}$ or ${}^5\text{A/T}$. The hydrogen bond between tyrosine 17 and ${}^3\text{guanine}$ can be formed with correct bond angles. The distance between the phenolic hydrogen and O_6 of ${}^3\text{guanine}$ is in the range of a normal hydrogen bond (approximately 3 Å). If the guanine is replaced by thymine the tyrosine 17 can form the correct angle for the hydrogen bond to a ${}^3\text{thymine}$ after turning around the $\text{C}_2\text{--C}_3$ axis. In both cases the plane of the phenolic ring lies in the same plane as the bases. II, Interaction of glutamine 18 and serine 21 with the second base pair ${}^5\text{T/A}$. Serine 21 donates its hydroxyl hydrogen to O_6 of ${}^5\text{thymine}$. The carbamide O of glutamine 18 can accept the N_6 hydrogen of ${}^3\text{adenine}$. Both hydrogen bonds can be formed with correct bond angles and distances. If we replace ${}^5\text{T/A}$ by ${}^5\text{G/C}$ the changed angles and distances allow either serine 21 or glutamine 18 to form hydrogen bonds, but not both. III, Interaction of aspartic acid 25 with the third base pair ${}^5\text{A/T}$. Aspartic acid 25 can act as an H-bond acceptor of the N_6 hydrogen of adenine. In the model the other oxygen of the carboxyl-group of aspartic acid is bound to histidine 29, which points backward with its ring. This interaction might exclude binding to cytosine. Histidine 29 can also interact with the $5'$ phosphate of this adenine. IV, Interaction of glutamine 26 with the fourth base pair ${}^5\text{T/A}$ or ${}^5\text{G/C}$. Glutamine 26 can form a hydrogen bond using its carbamide oxygen. The donor may be free N_6 hydrogen of ${}^3\text{adenine}$ or ${}^3\text{cytosine}$. Because of steric hindrance (the glutamine 26 side chain lies close to the alpha helix backbone), we favour the glutamine- ${}^3\text{adenine}$ bond rather than the glutamine- ${}^3\text{cytosine}$ bond. (If the fourth base pair cannot be recognized, hydrogen bonds to the fifth base pair ${}^5\text{C/G}$ or ${}^5\text{A/T}$ are also possible.)

Fig. 5 shows the model we built, Fig. 6 the interactions we believe to occur; Fig. 7 shows the DNA sequence we derived, which is short and partially ambiguous, although such a sequence is sufficient for specific recognition of the *lac* operator if we assume all four subunits of *lac* repressor to interact

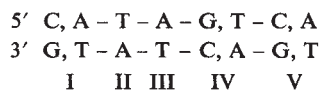


Fig. 7 DNA sequence recognized by the amino-terminus of *lac* repressor in the model. The sequence has been derived by model building. See text and Figs. 4-6.

similarly with DNA. The operator would then consist of four identical recognition blocks separated by (probably) identical spacers. We do not have to invoke any other region for contact. This restriction might be wrong, but it is unnecessary to invoke other regions to explain the specificity and binding energy of such a complex.

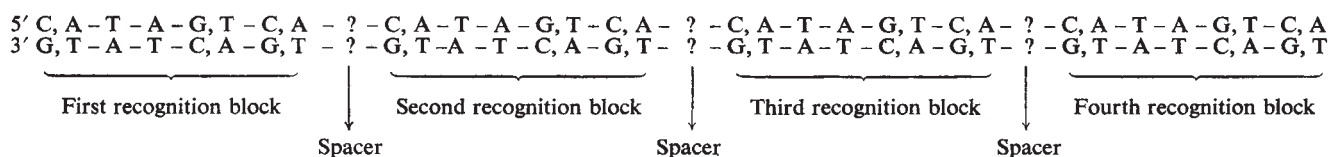


Fig. 8 Possible sequence of *lac* operator. We assume that all four subunits of repressor bind equally to operator. The DNA sequence of each recognition block is recognized by the N-terminus of one subunit of *lac* repressor. Each of the three hypothetical spacers consists of one to several base pairs. For a model that uses only two subunits see text.

Four lysines, four arginines and four histidines are certainly enough to explain the strong binding of *lac* repressor to *lac* operator. Eight base pairs recognized unambiguously and eight recognized ambiguously correspond to 12 base pairs recognized unambiguously. This is sufficient, as a sequence of 12 base pairs is unique on the chromosome of *E. coli*. Induction is easily explained quantitatively by the sequential distortion of the amino-terminal tails of *lac* repressor. Most enzymes do not have the symmetry which seems to be necessary for the protein to form such a complex. But again we do not know anything about the three-dimensional structure of proteins that bind specifically to DNA. Furthermore, we can only guess at the number of base pairs used as spacers. Models show that the spacer can be as small as one base pair. There is a hint favouring one base pair as spacer with *lac* repressor: Gilbert (personal communication) found the piece of DNA which is shielded by repressor against destruction by DNAase to be 22-23 base pairs long. Four recognition blocks consisting of five base pairs each, separated by a spacer of one base pair, would give 23 base pairs (Fig. 8). The sequence is furthermore sufficiently similar to poly dAT to explain the tight binding of repressor to this synthetic oligonucleotide²².

Recent mapping of operator-constitutive mutants has been interpreted to suggest a two-fold symmetry of the *lac* operator²³. If this is true only two subunits of *lac* repressor would interact with DNA. We can, of course, neither exclude nor prove that the amino-terminus has some complicated structure at the ends of the helical regions we invoke in binding which makes specific interaction with additional base pairs possible. A tetrahedral repressor would make additional demands on the structure of the complex.

Whether the details of the special model are correct or not for *lac* repressor-operator binding, the special model is

sufficient to explain all specific interaction between DNA and protein in *E. coli*. If all contacts between DNA and protein are made as in the special model, 128 ($4^{4/2}$) different DNA recognition blocks exist. Since the spacer can vary at least from one to six, $6 \times 128 = 768$ different DNA signals (operators etc.) can be constructed which can be recognized by tetrameric proteins. *E. coli* probably contains fewer regulatory systems than this.

The minimal model implies that only one or very few protein sequences (with regard to the hydrogen bond-forming amino-acid) exist which bind one particular DNA sequence. If this is true there must exist rules (a code) which describe the binding of protein sequences to DNA sequences. We suggest furthermore that these interactions were crucial in the evolution of self-replicating systems consisting of DNA and protein.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Dust in the Upper Atmosphere

DURING October 1971 we observed a large influx of dust into the upper atmosphere at heights above 30 km. The magnitude of this influx appears to be considerably greater than anything previously reported. At the time of writing (January 1972) dust is still being observed.

The observations were with a laser radar with transmitted energy of 3 J in pulses 2 μ s long; the repetition rate was 300 h^{-1} , and the wavelength of the radiation was 0.6943 μm . The receiver had a collecting area of 0.39 m^2 , and a bandwidth of 2.0 nm; its efficiency was 2%.

We first observed excess scattering on October 19; because of bad weather conditions no observations had been made for nearly a month prior to this date. The last profile obtained before the start of the dust influx was on September 22, and that gave a good fit to a standard atmosphere, with only slight deviations, probably caused by a tidal effect.

Some results of the observations are shown in Fig. 1 as the ratio of the observed scattering to that expected from a standard atmosphere. The standard atmosphere used is the US standard atmosphere (1966) for 15° N with a lowered mesopause temperature, providing a good fit to our last observation before the start of the dust influx. The measured profiles have been fitted to the standard atmosphere in the height range 30.5 to 32.5 km, so any dust at this height would decrease the apparent excess scattering.

The profile for October 19 (Fig. 1b) has a pronounced peak at 47 km. On the following day we observed this peak at 44 km; this is the only occasion where a well defined peak could be followed down in height. Assuming that this is the result of gravitational fall in a static atmosphere, we can estimate the particle size as follows.

The terminal velocity of small particles in the atmosphere is¹ $V(h) = 0.73g \, d \, r / \rho(h) \, c_m$ where g is the acceleration due to gravity, d is the density of the particle, r is the particle radius, $\rho(h)$ is the atmospheric density and c_m is the mean molecular velocity at height h in the atmosphere. This expression is valid provided the particle radius is much less than the mean free path for air molecules, which is about 20 μm at 45 km. We assume a particle density of 3.5 g cm^{-3} and use the measured fall velocity of 3.5 cm s^{-1} to obtain a particle radius of 1.2 μm , which is in the right size range for detection by optical radar. The corresponding radiation parameter, β , is 0.14.

On the first day on which dust was observed it was mostly concentrated in the height range 45 to 49 km, with a small secondary peak at 60 km. On subsequent days the dust was distributed over a wider range of heights. The average profiles for September, October (excluding October 19), November, December and January are shown in Fig. 2. There are a number of points of interest in this figure. First, we note a general decrease in excess scattering from November to January, indicating a residence time for the dust of the order of three months. Second, the minimum height at which dust is observed increases from about 35 km in October and November to 45 km in January, while the scattering from above 70 km is little changed. Although part of the excess scattering above 70 km might result from a poor choice of

atmospheric model, we are certain that the dust is removed most rapidly from the lower part of the atmosphere. This indicates considerable vertical mixing because if the particles were falling in a static atmosphere the region above 50 km would be cleared of micron size particles within a few days. The detection, by optical radar, of particles small enough to remain above 50 km for as long as one month, in a static atmosphere, would require an improbably large particle concentration.

The results suggest then that the dominant transport effect is not gravity but eddy diffusion. This invalidates our calculation of the particle size from the rate of fall. But atmospheric circulation will also affect the dust distribution. The only other measurements indicating the vertical transport of dust in this region of the atmosphere, that we are aware of, are those of Kent *et al.*², who observed an influx of dust from Comet Bennett. Their results appear to be in direct contradiction to ours. They first observed depletion of dust at heights above 70 km, 4 days after the initial influx. Unfortunately they appear to have made measurements for only one week after

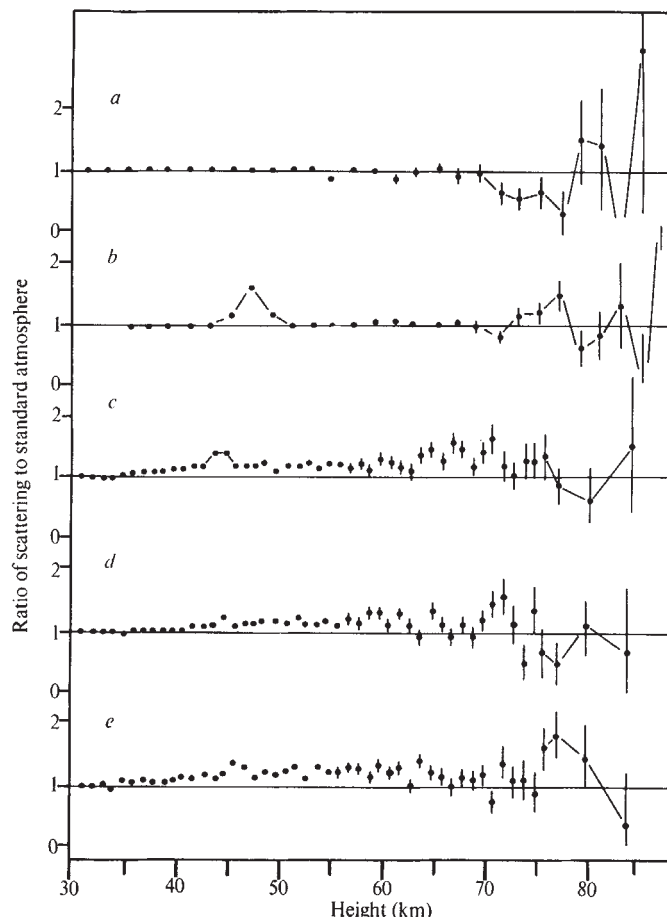


Fig. 1 Daily average scattering profiles observed at São José dos Campos (23° S, 46° W). a, September 22, 1971; b, October 19, 1971; c, October 20, 1971; d, October 21, 1971; e, October 22, 1971.

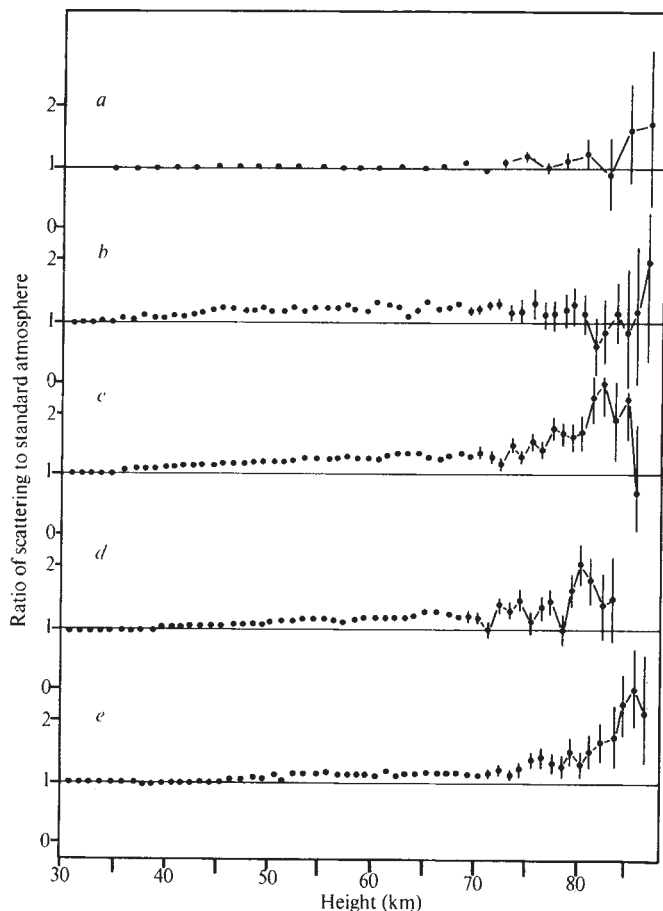


Fig. 2 Monthly average scattering profiles observed at São José dos Campos. *a*, September 1971; *b*, October 1971; *c*, November 1971; *d*, December 1971; *e*, January 1972.

the initial influx, and the total dust was much less than we observed. Over such a short time period atmospheric circulation might play a leading role in the redistribution of the dust.

For the profile of October 19, where the excess scattering can be measured fairly accurately, we can estimate the total amount of dust involved. In order to do this we must assume a size distribution, refractive index and density for the particles.

We shall base our calculations on the estimated particle size of 1 μm radius. But atmospheric particles are usually found to obey a Junge distribution of the type $dN(r)/d \log_{10} r = Cr^{-\gamma}$ where $N(r)$ is the differential particle concentration, r is the particle radius and C and γ are constants. So we have tried both assumptions, and find the total particle mass calculated on the basis of a Junge distribution with $\gamma=3$, extending from $r=0.1$ to $r=1.0 \mu\text{m}$, is less than twice that calculated for 1.0 μm particles. Assuming a refractive index of 1.5 we obtain a peak concentration of 500 particles m^{-3} at 47 km for the 1 μm particles, and for the Junge distribution we have nearly 10^4 particles m^{-3} with radii greater than 0.25 μm . The total density for the former case, assuming a particle density of 3.5 g cm^{-3} , is $7 \times 10^{-12} \text{ kg m}^{-3}$, and for the latter case, integrating across the distribution from 0.1 to 1.0 μm gives $10^{-11} \text{ kg m}^{-3}$. If the 47 km layer is equivalent to a 3 km thick block layer we obtain between 2×10^{-8} and $3 \times 10^{-8} \text{ kg m}^{-2}$. We believe that, provided most of the mass is concentrated in particles with radii between 0.1 and 1.0 μm , this estimate is good to within a factor of 3. If the dust contains appreciable mass in the form of particles outside this size range then our estimate is too low.

It seems unlikely that the dust was a local phenomenon, because although the initial 47 km peak vanished within a few days the total atmospheric dust does not appear to have decreased appreciably over a period of one month. Wind velocities at 45 km are typically of the order of 50 m s^{-1} so

we expect atmospheric circulation to disperse a dust cloud covering a small area of the Earth's surface within a few days. On the basis of a world wide influx we make a rough estimate of the total mass of dust involved by assuming that it occurred over one hemisphere; this gives a total mass of about 10^4 tonnes, which is of the same order as current estimates of the total daily influx of extra-terrestrial material of all forms.

B. R. CLEMESHA
Y. NAKAMURA

*Instituto de Pesquisas Espaciais,
São José dos Campos-SP-Brasil*

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Nature of the Cen X-3 System

THE X-ray flux from Cen X-3 shows large variations of amplitude with an average period of $\tau_0 = 4.85 \text{ s}$ (ref. 1), the precise value of the instantaneous period varying sinusoidally with a period $P = 2.09 \text{ d}$ (ref. 2). This variation in the periodicity of the amplitude variations is attributed to the Doppler effect, the source X being considered to be in a circular orbit around a massive body U. The orbital elements of X are

$$P = 2.08712 \pm 0.00004 \text{ d}$$

$$a \sin i = 1.191 (\pm 0.001) \times 10^7 \text{ km} = 17.1 R_\odot,$$

$$\frac{M_U^3 \sin^3 i}{(M_U + M_X)^2} = 3.074 (\pm 0.008) \times 10^{34} \text{ g} = 15.5 M_\odot.$$

The average value of τ is

$$\tau_0 = 4.842398 \pm 0.000001 \text{ s},$$

with the possibility of a secular decrease of 1 part in 4,000 over 4 or 5 months.

The average X-ray flux is about $10^{-8} \text{ erg cm}^{-2} \text{ s}^{-1}$ assuming that the observed spectral distribution fits a black body spectrum with $T = 3.3 \times 10^7 \text{ K}$, or a bremsstrahlung spectrum with $kT = 15.7 \text{ keV}$ and a cutoff at 3.8 keV. The average X-ray flux drops to about 10% of its usual value when X is in conjunction with U and on the other side of it relative to the Earth; the low level flux lasts for about 1/4 of the orbit period and during this stage the source does not show the usual marked variations with period 4.84 s, but remains quasi-constant. This evidence of an eclipse has been interpreted as implying that U is a spherical body of about $20 M_\odot$ with a radius of $13 R_\odot$. Both radius and mass are quite reasonable for a normal giant.

There are, however, difficulties in assuming that U is a more or less normal giant. A careful search by Elliot *et al.*³ did not reveal any star inside the error box of the X-ray position which varies with a period of about 4.8 s (down to $m_B = 16$) or any objects with negative colour index down to $m_B = 19$. Further, the region appears to be normal with respect to interstellar absorption. It is more likely that U would vary with the orbital period of 2.09 d, than with the 4.85 s period of X. No search for such variation has been carried out, and there is still the possibility that U is a relatively bright object which has escaped notice. Even so, it seems difficult to account for U as a normal giant. Assuming an inclination $i = 90^\circ$ (smaller values would lead to greater difficulties), the radius of U is $12.1 R_\odot$ and its mass $17.4 M_\odot$, if X has a mass of $1 M_\odot$. Taking two extreme values for the effective temperature gives the physical parameters of Table 1. These parameters are independent of the distance, which is unknown. Table 2 gives the apparent magnitudes which U should have at various distances if it were a K or a B star.

Table 1 Extreme Parameters for "Normal" Models of U

T_e (K)	Spectrum	L (erg s^{-1})	m_{bol}	m_B
4,000	KI	1.3×10^{35}	+0.9	+2.0
20,000	BC	8.1×10^{37}	-6.1	-3.5

Table 2 Apparent Magnitudes of U

d (pc)	m_B (K star)	m_B (B star)	L_x (erg s^{-1})	L_v (erg s^{-1})
1,000	12.0	6.5	1.2×10^{36}	1.8×10^{35}
1,500	12.9	7.4	2.5	3.6
2,000	13.5	8.0	4.8	7.0
2,500	14.0	8.5	7.5	1.1×10^{36}
3,000	14.4	8.9	1.1×10^{37}	1.6

A K star with a rather high, but not impossible, interstellar absorption might be reconciled with the observations if the distance is large enough.

But because of its proximity to X, U absorbs a large fraction (14.6%) of the energy emitted by X. In other words, besides its own energy sources—corresponding to a normal star—U has an external source, the contribution of which cannot be ignored. Table 2 also shows the power, L_x , which X must emit at various distances, in order to give the observed flux at the Earth, and $L_v = 0.146 \times L_x$, the power absorbed—and, of course, re-emitted—by U. This shows that U cannot be an almost normal K giant, because the power it receives from X is much larger than or of the same order of the power radiated by a K star of its radius, unless the whole system is very close to the Earth, so that the total X-ray radiation from X is very small. But then U should appear very bright because of its own energy sources.

It seems that the only solution is to assume a very large obscuration—10 mag or more—by a local cloud. In this case Cen X-3 would consist either of a relatively close K giant or of a more distant B star, with a pulsating X-source orbiting it; the pair should be enveloped by a large, thick cloud of dust, which almost entirely absorbs the optical radiation of the star and is transparent to the X-ray radiation of the source.

I do not see any strong reason to reject this explanation, except that it looks rather artificial. Obviously the system should be a powerful infrared source; the observation of strong infrared emission from Cen X-3 might perhaps settle the question.

Meanwhile, it is not unreasonable to explore other possibilities; an attractive one is a model in which the eclipsing body is provided by the action of X on some diluted matter connected with the system. An interesting model is to assume that the central mass U is a dark massive highly collapsed body—a black hole—of some $20 M_\odot$; X is orbiting around U according to the X-ray observations, but the eclipse is caused by a hot cloud of highly ionized gas lying between X and U and rotating synchronously with X.

The simplest way to achieve this is to assume that around U and inside the orbit of X there is a disk or ring of matter—possibly a remnant of matter ejected in the equatorial plane by U during the collapse. The plane of the ring is coincident with the orbital plane and the ring itself is in a state of differential rotation around U; the part of the ring facing X is brought to a very high temperature by the radiation of X and becomes opaque, thus screening the rest of the ring which remains rather cold. Because the ring rotates with an angular velocity different from that of X, the hot cloud thus formed is not a definite body, but rather an ionization wave driven by the motion of X; the cooling time is very short. In this way the obscuring matter always lies between X and U and eclipses X at the right time for an observer at the Earth. If the eclipsing cloud is sufficiently thin optically to its own radiation, the

equilibrium condition gives

$$V\varepsilon = L_v \quad (1)$$

where V is the volume of the cloud and ε the volume emissivity. Equation (1) states that the incoming flux is absorbed by the cloud and re-emitted in all directions, so that very little, if any, of the original (pulsed) flux from X comes through. The re-emitted X-ray photons are, in this picture, responsible for the residual flux observed during the eclipse; they will also contribute an almost constant flux which adds to the pulsed flux at all times. The fact that the observed residual intensity during the eclipse is somewhat lower than 15% of the average may indicate that the solid angle of the obscuring cloud to the source is somewhat smaller than that corresponding to a spherical body with the same duration of the eclipse.

Another condition is that the cooling time must be long relative to the pulsation period in order to damp out the short period variations of the source, and short relative to the orbital period in order to give relatively sharp eclipses. If T_e is the temperature of the cloud and N_e the electron density, this gives, for the cooling time,

$$\tau_{\text{cool}} = \frac{3}{2} \frac{k T_e N_e}{\varepsilon} \simeq 100 \text{ s} \quad (2)$$

assuming that there is no hydrogen left, so that the electrons greatly outnumber the heavy particles present in the ring. In these conditions ε must be essentially due to bremsstrahlung; by integration over all frequencies and omitting the Gaunt factor, one obtains⁴

$$\varepsilon = 1.5 \times 10^{-27} Z N_e^2 T^{1/2} \text{ erg cm}^{-3} \text{ s}^{-1} \quad (3)$$

where Z is the charge number of the heavy particles. Thus, there are two conditions for three unknowns V , T_e and N_e , so that there is a certain freedom for the choice of the parameters; this can be limited on the following grounds. The perturbation induced by the mass of X on the motion of the ring will cause a gap at a distance from U corresponding to an orbital period 1/2 that of X, similar to the divisions of Saturn rings. This suggests that the cloud is more likely to lie outside a circle with a radius $2^{-2/3} \times a = 7.5 \times 10^{11} \text{ cm}$. If the cloud is spherical, its radius must be $0.153 a = 1.8 \times 10^{11} \text{ cm}$ to produce an eclipse of the right duration. The nearest point of the spherical surface to X is then only $0.75 \times 10^{11} \text{ cm}$ from the centre of X. This raises some problems. The inner Lagrangian point corresponding to the masses concentrated in U and X lies at about $0.24 a = 4.6 \times 10^{11} \text{ cm}$ from X (see, for instance, ref. 5) which is well inside the cloud if it is spherical. This means that at least part of the cloud will tend to fall onto X or to orbit around it rather than around U. Also the cloud will be exposed to a great radiation pressure, because of the proximity of the X-source.

The two effects in some way compensate each other, but the obvious instability which arises deserves further investigation. For a very crude estimate, however, I shall ignore this difficulty and adopt for the cloud a radius of $1.5 \times 10^{11} \text{ cm}$, or a volume of 10^{34} cm^3 .

There is still another free parameter, namely the distance d of the system. Table 3 gives the physical parameters of the cloud for $d = 500$ and $d = 1,000 \text{ pc}$, respectively.

A cloud of about $1.5 \times 10^{11} \text{ cm}$ radius, $T_e = 10^7 \text{ K}$, $N_e = 5 \times 10^{11} \text{ cm}^{-3}$ satisfies the conditions; the total mass is $2 \times 10^{22} \text{ g}$. This is a very small mass indeed; hence although the lifetime of the cloud may be rather short, because of the

Table 3 Parameters of Eclipsing Cloud as Function of Distance

d (pc)	L_v (erg s^{-1})	ε ($\text{erg cm}^{-3} \text{ s}^{-1}$)	T_e (K)	N_e (cm^{-3})
500	0.45×10^{35}	2.5	4.4×10^6	3.6×10^{11}
1,000	1.8×10^{35}	13	1.2×10^7	5.2×10^{11}

various instabilities arising from mechanical as well as from thermal reasons, it is not unreasonable to think that it may be continuously reformed by the matter of the ring for quite a long while, without the mass of the ring itself being unduly large.

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LIVIO GRATTON

University of Rome,
Italy

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Plate Tectonics and Orogenic Belts in the South-east Pacific

GEOLOGICAL similarities suggesting the "reappearance" of the South American Andes in the Antarctic Peninsula were first noted at the turn of the century¹⁻³. Further work⁴⁻¹¹ was consistent with the view that the South American Andes and Antarctic Peninsula orogens have a common history since at least the Late Palaeozoic, and are to be regarded as parts of an originally continuous and rectilinear cordillera that was subsequently bent and disrupted to form the Scotia Arc. Drift reconstructions¹² show this proto-cordillera bordering the Pacific margin as an uninterrupted orogenic belt throughout the Mesozoic and Early Cainozoic. Here, I question this, on the basis of a recent analysis¹³ which shows that the two orogens have developed under widely different tectonic conditions, and must have had different settings with regard to plate boundaries since the Late Jurassic.

After the Late Palaeozoic to Early Mesozoic Gondwanide Orogeny¹⁴, both regions possibly formed part of a continuous continental landmass. Subsequent volcanism¹⁵ of calc-alkaline andesitic to rhyolitic composition affected extensive areas across this continent^{16,17}. In the Late Jurassic–Early Cretaceous a mioeugeosynclinal couple and volcanic island arc¹⁸ formed in the west of South America in a direction oblique to the Gondwanide structural grain¹⁹⁻²², indicating regional subsidence, extension and rifting along a newly established continental margin. The situation may have resembled the present marginal seas behind island arcs in the western Pacific, where new oceanic crust is being formed²³⁻²⁶. The mafic belt associated with a typically eugeosynclinal sediment assemblage in Magallanes^{18,27} (volcanic greywacke, argillite and radiolarian chert several thousand metres thick) may represent oceanic crust of an ancient marginal sea¹³. Geosynclinal orogenesis proceeded in three stages separated clearly in time and space¹⁸, geosynclinal polarity thus being associated with orogenic polarity²⁸; migration eastwards towards the foreland incorporated successive flysch and molasse basins in the fold belt¹⁸. Tectogenesis was alpinotype^{7,29}, and was accompanied locally by high-grade metamorphism and migmatization^{18,30}. Evidence of a continued orogenic regime and setting along an active continental margin is provided by recent calc-alkaline andesitic-dacitic volcanism in the cordillera³¹, its taphrogenic breakdown along the Pacific^{20,32} and its association with a deep-sea trench with characteristic gravity anomaly³³ and a floor of normal oceanic crust³⁴.

In striking contrast the Antarctic Peninsula, during its post-Gondwanide history, was not involved in geosynclinal oro-

genesis²⁸. Sedimentation during Jurassic–Early Cretaceous time was in local and partly fault-bounded post-orogenic basins (residual, intermontane, back deeps³⁵) where various molasse-type, terrestrial to epicontinental shallow marine sequences of limited thickness accumulated^{13,35}. The orogenic stage is marked by widespread granodioritic plutonism³⁶ (mainly at about 100 m.y.³⁵) accompanied by regional uplift; deformation is germanotype, fault-block tectonics prevailing except at the base of the Peninsula where local diastrophism caused tight folding prior to the mid-Cretaceous intrusions^{37,38}. Reports of intense post-Aptian thrusting from the west in Alexander Island³⁹ are not supported by critical examination of the data^{13,35}, and the recognition of multiple deformation at Cape Legoupil⁴⁰ seems to be based on rocks of the Late Palaeozoic Trinity Peninsula Series¹³, which was affected by the Early Mesozoic Gondwanide Orogeny^{17,35,41}. Late Senonian external molasse sequences⁴² remained almost undeformed; no Tertiary sediments of importance have accumulated in the Antarctic Peninsula and surrounding islands¹⁷. In the Cainozoic further uplift with faulting and chiefly longitudinal rifting⁴²⁻⁴⁴ created the principal relief, and widespread volcanism produced mildly undersaturated basaltic rocks⁴⁵. Thus no orogenic regime appears to be affecting this region.

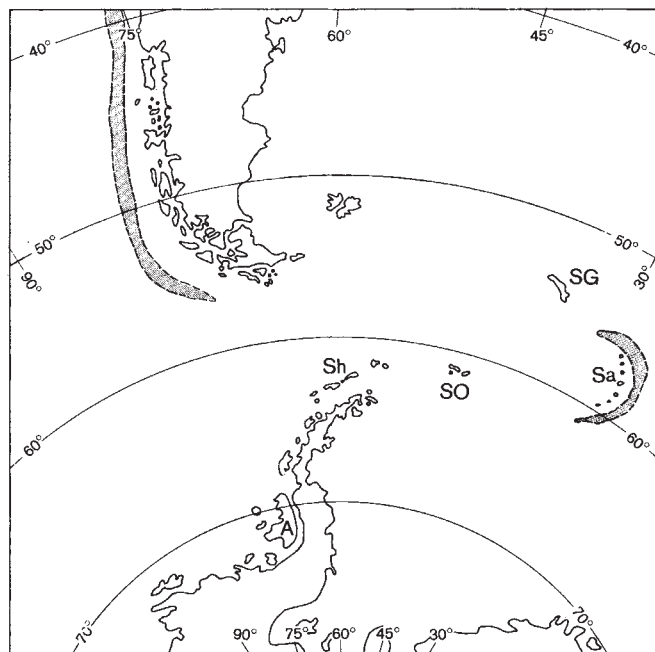


Fig. 1 Southern South America and Antarctic Peninsula with Islands of the Scotia Arc (SG: South Georgia; Sa: South Sandwich; SO: South Orkney, Sh: South Shetland). A: Alexander Island. Grey shading: oceanic trenches.

I conclude that the Andean belt in southernmost South America is an accretionary belt and a continental margin feature. Its setting is not related to Gondwanide tectonics. The Antarctic Peninsula orogen is an intracontinental feature of essentially Gondwanide inheritance; the regional tectonic elements are parallel to the older Gondwanide trend, and tectonic events are characteristic of post-orogenic developments reflecting cratonic consolidation.

In terms of plate tectonic theory, the evolution of the Andean belt is related to plate junctures; they reflect complete tectonic reorientation in post-Gondwanide time. There are indications of continuing (or renewed) operation of a subduction zone beneath the mountain chain, but the buried nature of the trench^{33,34}, lack of seismicity⁴⁶ and sparsity of volcanism suggest that subduction is not very active at present. In the Antarctic Peninsula, however, the tectonic framework and evolution of the Late Mesozoic–Cainozoic belt appears not to

be related to plate junctures. Although magnetic evidence⁴⁷ suggests consumption of crust during much of the Late Tertiary along the shallow trench north-west of the South Shetland Islands⁴³, the region's present position is in plate interiors with structural trends apparently oblique to modern plate boundaries⁴⁷. Thus whereas the Pacific continental margin along the southern Andes marks the boundary between South American and Pacific-Antarctic plates, the Pacific continental margin adjacent to the Antarctic Peninsula (and all of Western Antarctica) is not a plate boundary; it is an Atlantic-type margin²³. From geological evidence, a similar situation must have prevailed through most of the Late Mesozoic and Cainozoic. Thus oceanic crust as old as Jurassic could be expected in the Pellinghausen Sea west of the Antarctic Peninsula. This seems to be supported by recently discovered magnetic anomaly lineations⁴⁸.

Comparison of tectonic setting and framework, sedimentary and orogenic history and structure, together with a plate tectonic interpretation, shows that the orogenic belts of southernmost South America and Antarctic Peninsula are not related to each other. Thus, the term "Antarctandes"⁴⁹ is not justified and is misleading³⁵. As a corollary the problem of the Scotia Arc is not limited to two alternatives (oroclinal bending and disruption of an originally straight orogenic belt¹⁰, or constitution of the arc as a primary feature^{47,50}); the Scotia Arc may be a composite feature with limbs of different origin and partial bending only. There can be little doubt that originally South Georgia was linked with, and nearer, South America^{27,51}, and that it rotated to its position, perhaps along faults trending north-east⁷ or east-west¹¹, or probably by a combination of events. The pronounced virgation of structural elements at the southern tip of South America⁷, already noted by Suess³, indicates that any bending and disruption was not a simple process and did not entirely post-date orogenesis. On the southern limb the South Orkney Islands are closely related, on the basis of rock content, geological history and structure^{17,52}, to the Antarctic Peninsula region; they may have been displaced by complex fragmentation and rotation^{53,54}. Finally, the South Sandwich arc-trench system is a more recent feature built on oceanic crust⁴⁵, and apparently belongs to a new crustal plate entirely of its own⁴⁷. Thus although oroclinal bending may have occurred to some extent particularly in the Andean belt, I envisage the curvature of the Antarctic Peninsula orogen as possibly primary; this view is based on the fact that its trend is parallel to and inherited from Gondwanide tectonics, "frozen-in" as it were in plate interiors since at least the Late Jurassic. The limited palaeomagnetic evidence available (I. Dalziel, personal communication) supports this conclusion.

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H. R. KATZ

DSIR NZ Geological Survey,
Lower Hutt,
New Zealand

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Network Thermodynamics

We would like to thank Professor H. Busse for bringing to our attention two inaccurate statements in our recent communication¹. The paragraph on reciprocity concludes with the following statements: "A system composed of reciprocal components will exhibit only 'trivial' dynamic behaviour, that is, monotonic decay to a stationary point. Oscillations and amplification are not possible with reciprocal systems." Thus stated, the first of these assertions is misleading, and the second is false. What was intended is the following.

For reciprocal systems which can be described by a complete set of variables, Brayton and Moser² have derived a "mixed potential function". Because the differential equations required for modelling most biochemical phenomena involve only

resistive and capacitive elements, this mixed potential takes the particularly simple form

$$G^* = \int_0^e f(e)^T de \quad (1)$$

where G^* is the cocontent³.

The equations of motion are given by

$$C(e)\dot{e} = -\text{grad } G^* \quad (2)$$

Equation (2) implies that all solutions of the network equations flow downhill on the potential G^* , that is,

$$\frac{d}{dt} G^* = -\dot{e}^T C(e) \dot{e} \quad (3)$$

Because the local equilibrium postulate guarantees that the capacitor constitutive relations are positive definite, $d(G^*)/dt < 0$ except at the system equilibria where $\dot{e} = 0$. If

(G^*) tends radially to $+\infty$, which it usually does for large values of thermodynamic forces and fluxes (equation (1)), then all solutions are bounded, and all initial conditions will yield solutions which tend toward the singular points of (2) as $t \rightarrow \infty$. Although along solutions (G^*) decreases monotonically in time, the functions $e_i(t)$ (that is, the solution) need not decay in a monotone fashion to the equilibrium set as originally stated. But the existence of a unique limit set excludes the possibility of sustained relaxation oscillations. For $R-C$ systems reciprocity alone eliminates the possibility of linear harmonic oscillations, because all eigenvalues are real, albeit not necessarily negative, of course. The singular points of (2) corresponding to a minimum of (G^*) will be stable, and the others (corresponding to a maximum or saddlepoints) will be unstable^{4,5}.

Although the universe of possible dynamic behaviour is certainly not "trivial" (an unfortunate and coercive term), the restriction of reciprocity severely constrains the dynamic possibilities of a biological system. Moreover, there seems to be no *a priori* reason for imposing this restriction; indeed, most chemical reactions are not reciprocal.

The second assertion concerning amplification disregards the possibility of negative resistance and/or parametric amplification, a distinct possibility in, say, excitable tissue. But most amplification schemes do indeed depend on nonreciprocal constitutive relations (for example, transistor amplifiers). Once again, the modelling possibilities are greatly enriched by the dropping of the near equilibrium reciprocity requirement.

G. OSTER

Mechanical Engineering Department and
Donner Laboratory,
University of California, Berkeley

A. PERELSON

Donner Laboratory,
University of California, Berkeley

A. KATCHALSKY

Weizmann Institute of Science, Israel, and
Donner Laboratory,
University of California, Berkeley

Received February 28, 1972.

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Neutron Activation Analysis of Aegean Obsidians

THE wide extent and early date of the prehistoric trade in obsidian in the Aegean has been documented over the past decade¹⁻³. Trace-element analysis by optical spectroscopy did not, however, achieve an entirely satisfactory separation between all the sources of obsidian which might have been used in the prehistoric Aegean. Fission track analysis has recently been employed³ to corroborate the early trade in obsidian from Melian sources. The technique of neutron activation analysis has now been utilized to discriminate between the products of the various natural sources of the material in the Aegean, central Anatolia and central Europe. The analysis of artefacts from archaeological contexts then allows important new inferences to be drawn about the extent of the prehistoric obsidian trade in those regions.

The principal natural sources of obsidian in the Aegean are those on Melos, a Cycladic island 160 km north of Crete. The other Aegean sources are Antiparos, 60 km north-east of Melos and Giali, 240 km to the east. The relevant Anatolian sources are at Acigöl and Çiftlik, 900 km to the east of Melos, while the central European sources, in Slovakia and northern Hungary, are 1,200 km to the north-north-west.

The investigations of Gordus and his co-workers⁴⁻⁶ have shown the value of neutron activation analysis in the characterization of obsidian, both in North America and the Near East, where the results agree well with those obtained by optical spectroscopy^{1,7}. The technique of neutron activation analysis using Ge-Li detector systems is now well known and is summarized here only to clarify the particular parameters used. Fragments of obsidian ranging in mass from 120 to 2,700 mg were cleaned of surface contamination by treatment with hydrofluoric acid, washed with high purity water and, after drying, wrapped in aluminium foil. Batches of samples and appropriate standards were irradiated in the Herald reactor of AWRE, Aldermaston, for 48-h periods at a thermal neutron flux density of 1.8×10^{12} n cm⁻² sec⁻¹. The intense radioactivity of the specimens on removal from the reactor was ascribed primarily to ²⁴Na so the first examination took place four days later when the short-lived γ -ray activity was investigated. After a further twenty-one days, samples were re-examined for the now dominant long-lived isotope activity. Analysis was carried out using a 70 cm³ Ge-Li crystal γ -ray detector with associated amplifiers and a 1,024 channel data accumulation system. All spectra were recorded on punched tape for subsequent processing by a computer program formulated to identify and evaluate specific activities of individual radio-isotopes. Comparison of these with corresponding activities in standards gave element concentrations. The following elements were identified in all obsidian specimens examined: Na, Cs, Rb, U, Th, Sc, Ta, Co, Fe, La, Sm, Hf, Ce, Eu, Tb, Yb and Ba, Zn, Sb, Zr, Lu. Quantitative estimates were not possible for the latter group owing to intense interference. The sixteen for which concentrations could be assessed are in the first group. These data, for samples from each known source, were analysed by the method of Newton and Spurrell⁸. We found that complete discrimination amongst sources could be obtained using the elements Cs, Ta, Rb, Th, Tb, Ce and Fe provided all concentrations were expressed relative to the concentration of Sc. This relative representation was necessary to reduce uncertainties arising from considerable variations in counting "geometry", due to the widely varying shape and mass of the samples. Scandium was chosen as an internal standard because, for obsidian, it can be assessed with high accuracy.

The source discrimination obtained is shown in Fig. 1a where

$$\frac{1}{Sc} \left(Cs + Ta + \frac{Rb}{100} + \frac{Th + La + Ce}{10} \right)$$

is plotted as ordinate against Fe/Sc on an arbitrary scale. An

adequate separation is obtained for all the sources, although the number of samples examined in this preliminary work is small; thus the statistical evidence for the source discrimination is not yet strong and further detailed investigation is shortly to be undertaken. It is supported by the close correlation displayed by analyses of material from archaeological contexts (Fig. 1b). The new discrimination between the two Melian sources at Adhamas and Dhemenegaki, only 10 km apart, is of particular interest.

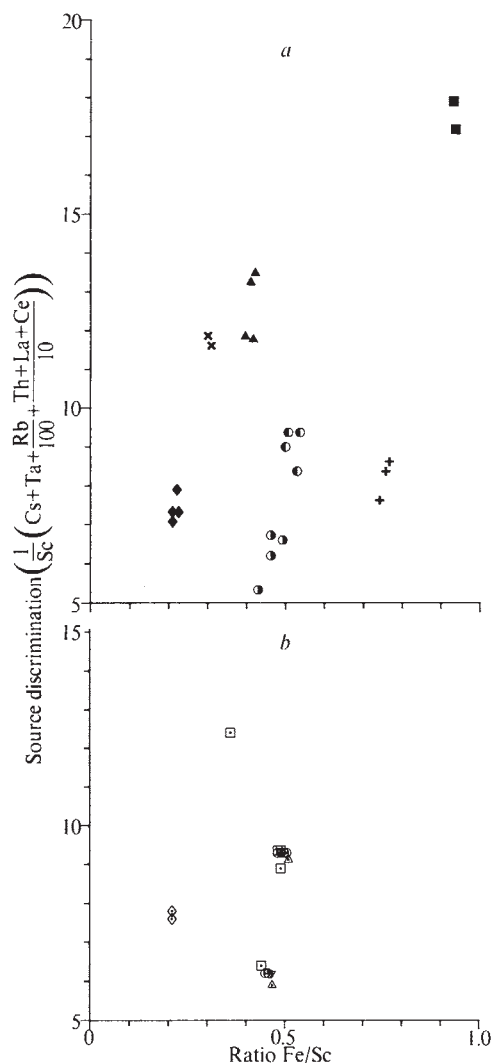


Fig. 1 Element concentration patterns for obsidian from sources and occupation sites in the Aegean. *a*, NAA obsidian sources: ●, Melos "A"; ○, Melos "D"; ▲, Giali; ■, Acigöl; ◆, Carpathian; ×, Çiftlik; +, Antiparos. *b*, Occupation sites: ○, Franchthi; □, Sitagroi; ◇, Vinča; △, Nea Nikomedeia; ▽, Kea.

The artefact analyses contribute to the understanding of prehistoric trade in the Aegean in several ways.

(1) They confirm that two pieces of obsidian from neolithic Vinča near Belgrade, from the fifth millennium BC, are of central European origin. No obsidian of Aegean origin has yet been identified north of Greece.

(2) The widespread early use in the Aegean of Melian obsidian is well documented, no central European obsidian being recognized in the Aegean. Nea Nikomedeia and Sitagroi are in Macedonia in north Greece, and represent the most northerly extent of the Melian distribution. Franchthi is in south Greece, and four of the samples analysed were from mesolithic levels dated around 7000 BC in radiocarbon years. It is of particular interest that while both the Melian sources

were later in use, all four of these very early specimens had been brought from the source at Adhamas.

(3) Obsidian originating from the central Anatolian source at Çiftlik has now been identified at the settlement mound of Sitagroi in north Greece. The find was not well stratified but probably dates from the early bronze age, in the third millennium BC. Hitherto obsidian from Çiftlik has been documented in the Aegean only by finds from Knossos in Crete⁷. The Sitagroi analysis thus gives further definite evidence of prehistoric trade or contact between Macedonia and central Anatolia, a finding of real interest for Aegean prehistoric archaeology.

A. ASPINALL

School of Applied Physics,
Bradford University

S. W. FEATHER

City of Bradford Art Gallery and Museums

C. RENFREW

Department of Ancient History,
Sheffield University

Received March 13, 1972.

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BIOLOGICAL SCIENCES

Specific Heat Measurements of Poly-(L-Alanine) at Low Temperatures (1.8–20 K) and Aspects of One Dimensionality

HOMOPOLYPEPTIDES have long been considered as model systems for the study of protein conformation^{1,2}, and synthetic homopolypeptides are now available commercially. Solid state and polymer physics have advanced to where the simpler homopolypeptides can be treated theoretically by lattice dynamics and experimentally by the physical techniques of neutron, infrared and Raman spectroscopy, which are more suited to the examination of the optical vibrational modes of a molecule which are of energy higher than, say, 70 cm⁻¹. Specific heat methods, on the other hand, have the advantage that by measuring at low temperatures (1–20 K) only the acoustic modes are appreciably excited thermally (a temperature of 1 K corresponds approximately to 0.7 cm⁻¹). Moreover, the specimen need not be in single-crystal form, and at low temperature the vibrational spectrum is easier to interpret, and the useful picture of Debye³ holds well. The specific heat measurements reported here are believed to be, by a factor of 100, the lowest temperature specific heat measurements made on a biopolymer⁴. Such measurements can give information about the proportion of a protein which is in alpha-helical or beta-sheet structure, and also will be necessary in evaluating the basic thermodynamics of enzyme reactions⁵.

Basically, the one dimensional (intra-molecular) nature of a high polymer gives a specific heat proportional to temperature (*T*), while its three dimensional characteristics give a contribution proportional to *T*³. (The polymer can be simply imagined as many beads on lengths of chain, with beads in

adjacent chains sticking slightly to one another.) The combination of the two contributions was evaluated by Tarasov^{6,7} involving effective Debye temperatures θ_1 (one dimensional) and θ_3 (three dimensional), which give a measure of the appropriate one and three dimensional elastic force constants in an assembly of polymeric molecules.

We have measured the low temperature specific heat of an apparently random sample of poly(L-alanine) (PLA, Pilot Division of New England Nuclear Corp., Mass.), that is, poly($-\text{NH}-\text{CHR}-\text{CO}-$) with $\text{R}=\text{CH}_3$, which is structurally the simplest homopolymer after poly(glycine) ($\text{R}=\text{H}$). The structurally simplest polypeptides, particularly those of high molecular weight, are insoluble in commonly used reagents, leading to many difficulties in analysis. The limiting viscosity number of PLA in dichloroacetic acid was 68 ± 2 ml./g (E. K. Lai, personal communication) implying (from poly-DL-alanine)⁸ a molecular weight of approximately 19,000 or a degree of polymerization of 270. Preliminary ultracentrifuge measurements by Professor W. R. Bauer (Department of Chemistry, Colorado University) suggest that the PLA is quite poly-disperse in molecular weight. Low angle (up to 3°) X-ray scattering measurements by Dr D. H. Reneker (Polymer Crystal Physics Division, National Bureau of Standards) show no fibre or crystalline structure, and are typical of those of amorphous polymers. Infrared absorption measurements ($400-4,000\text{ cm}^{-1}$) give no evidence of alpha (helix) or beta (sheet) structure⁹.

To measure specific heat, C , one applies an amount of heat ΔQ to a thermally insulated sample and measures the resulting temperature rise ΔT . Then $C = \Delta Q / \Delta T$. The specific heat apparatus is described elsewhere¹¹. The ratio (heat capacity of PLA)/(heat capacity of holder, copper and thermometer) was 1.9 at 2 K, 3.5 at 10 K and 1.7 at 20 K.

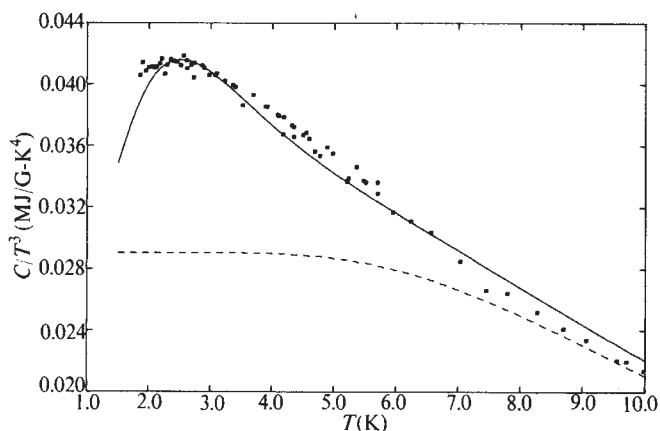


Fig. 1 Specific heat (divided by temperature cubed, for scaling) of random poly(L-alanine) 1–10 K. The broken curve is a fit to the Tarasov model; the full curve is a fit to the Tarasov model with one Einstein mode.

The results above 10 K are easily fitted to the model^{6,7}, showing definite one dimensional and three dimensional molecular contributions to the specific heat. The gram-molecular weight of the vibrating unit was taken as 71.08. At lower temperatures, the experimental data (Fig. 1) give evidence for another contribution to C . Such anomalies have been observed in other polymers and glasses^{11–15}, and have been accounted for by assuming that a small mole-fraction, f , of the total vibrational modes are Einstein oscillators of characteristic temperature θ_E ^{11,12} (possibly due to lattice resonant modes^{11,12}). The corresponding fit (Tarasov model plus Einstein oscillator) for our data (Fig. 1) is quite good (s.d. = 3.6%) giving parameters

$$\theta_1 = 323 \text{ K (2.4\%); } \theta_3 = 54 \text{ K (4.2\%); } \theta_E = 12.4 \text{ K (1.2\%); } f = 0.0032 \text{ (0.4\%).}$$

The numbers ($x\%$) are the change in the s.d. of the total specific heat fit when a parameter value is increased by 5% from the values quoted.

Lattice dynamical calculations of C have been made for alpha-PLA^{16,17}. No similar studies of amorphous PLA—which is a solid state equivalent of the random coil in the helix-coil transition—seem to have been made, although related problems^{18–20} have been treated.

These results illustrate that specific heat measurements at low temperatures of polypeptides can be useful for the measurement of molecular elasticity. The interpretation will be valid at room temperature (provided there are no unexpected conformational changes). Comparisons can be made with vibrational spectra from theoretical calculations. This work has obvious extensions to other biopolymers.

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Note added in proof. We have extended this work to predominantly alpha and beta PLA (*Nature* and *Biopolymers*, in the press). P. Delhaes, M. Daurel and E. Dupart have done similar work (*C R Acad. Sci. Paris*, in the press).

LEONARD FINEGOLD*
JESSE L. CUDE†

Department of Physics and Astrophysics,
University of Colorado,
Boulder, Colorado 80302

Received June 21, 1971; revised January 31, 1972.

* Present address: Department of Biology, University of California, San Diego, La Jolla, California 92037

† Present address: Palo Verde College, Blythe, California 92225.

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Is "Orbital Steering" a New Concept?

INTRAMOLECULAR reactions have, for many years, been used as models for the productive ES complex in enzymatic reactions^{1–3}. Chemists have sought explanations for the rapid rates in certain intramolecular reactions hoping that, by understanding these factors, much could be gained towards the understanding of the efficiency of enzymatic catalysis.

Table 1 Relative Rates of Anhydride and Lactone (Ester) Formation

	Anhydride formation ⁹	k_{rel}	Lactone (ester) formation ⁴	k_{rel}
1a	$CH_3COO^- + CH_3COOC_6H_4Br(p)$	1.0	1b $CH_3COOH + EtOH$	1.0
2a		$\sim 1 \times 10^3$ M	2b	0.79×10^2 M
3a		$\sim 2.2 \times 10^5$ M	3b	6.6×10^3 M
4a		$\sim 5 \times 10^7$ M	4b	1×10^6 M

The term "orbital steering" was introduced by Koshland and co-workers to convey the concept that much of the facility of enzymatic reactions can be explained on the basis that the active sites of enzymes are so constructed that they align ("steer") the orbitals of substrate and catalytic groups optimally to enter the transition state^{4,5}. Without questioning the validity of this concept⁶ or the assumptions made by Koshland^{7,8} and co-workers in their quasi-theoretical treatment, what new factors are introduced by "orbital steering" to the arguments concerning steric acceleration in intramolecular reactions and, by implication, into enzymatic catalysis as well? If one examines experimental results it is obvious that "orbital steering" is a term provided for a well known hypothesis¹⁻³ (Table 1). On the right hand side of Table 1 are the experimental results of Storm and Koshland⁴ which were used to substantiate the importance of "orbital steering"; on the left hand side are the results of Bruice and Pandit⁹. The similarity of the experimental approach is most remarkable as are the similarities in rate enhancements. From the conclusion of the Bruice-Pandit paper: "These results point to the tremendous enhancement of rate that an enzyme could achieve by fixing the reacting species in a steric conformation closely resembling that of the transition state for the reaction", meaning that interacting orbitals must be aligned. Rather than providing a new concept in biochemistry, the study of Storm and Koshland is more an application of an existing experimental approach to provide an interesting but additional example.

The present controversy surrounding the explanation of the facility of enzymatic catalysis revolves around the quantitative importance of three proposals. The first two involve ground state phenomena and the third transition state stabilization. They are: (1) the free energy of the ground state is sufficiently raised by bringing together (elimination of translational freedom) and sterically aligning substrate and active site functional groups to enter the transition state; (2) the ground state free energy is sufficiently raised from loss of translational and rotational entropy on formation of a tight productive ES complex⁶; and (3) the transition state free energy is sufficiently lowered due to preferential binding of transition over ground state¹⁰.

T. C. BRUCE

Department of Chemistry,
University of California,
Santa Barbara

Received December 6, 1971.

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Cyanate Inhibition of Erythrocyte Glucose-6-phosphate Dehydrogenase

It has been suggested that cyanate (CNO) may be useful as a therapeutic agent in the prevention of occlusive sickle cell crises¹. Cyanate irreversibly carbamylates the N-terminal amino (NH₂) groups of proteins and the free epsilon -NH₂ groups of lysine². Carbamylation of the terminal valine residue of sickle haemoglobin is thought to be responsible for preventing affected red blood cells (RBCs) from sickling when they are deoxygenated¹. Sick cell erythrocytes incubated with 50 mM cyanate for 90 min *in vitro* reportedly have an increased survival when they are radiolabelled and reinfused into the donor³. This report describes a possible side-effect of RBCs treated *in vitro* with sodium cyanate in similar experimental conditions: there is irreversible inactivation of the enzyme glucose-6-phosphate dehydrogenase (G6PD).

Erythrocytes from healthy human adults were collected in EDTA and washed three times with a pH 7.4 phosphate-buffered salt solution (PBS) containing 110 mM NaCl, 20 mM Na₂HPO₄, and 4 mM KH₂PO₄. After the final wash, a 20% RBC suspension was made, and glucose added to a final concentration of 10 mM. When neutralized sodium cyanate was used, an equivalent amount of chloride was withheld to maintain a constant anionic strength. Aliquots of the reconstituted erythrocyte suspension were incubated at 37° C in a Dubnoff metabolic shaker at 100 oscillations per minute. Established methods were used to measure reduced glutathione (GSH), oxidized glutathione (GSSG), and G6PD⁴. Hexosemonophosphate (HMP) shunt activity was studied by measuring the oxidation of glucose-1-¹⁴C to ¹⁴CO₂ (ref. 5). This pathway was evaluated under resting conditions, during a peroxide stress with 20 mM sodium ascorbate, and with 0.2 mM methylene blue. Ascorbate produces H₂O₂ after reacting with oxyhaemoglobin. Methylene blue transfers electrons from dihydronicotinamide adenine dinucleotide phosphate (NADPH) to oxygen. Both of these reactions increase nicotinamide adenine dinucleotide diphosphate (NADP) availability, and thereby stimulate the HMP shunt (Fig. 1)^{6,7}. GSH stability was determined by measuring the changes in

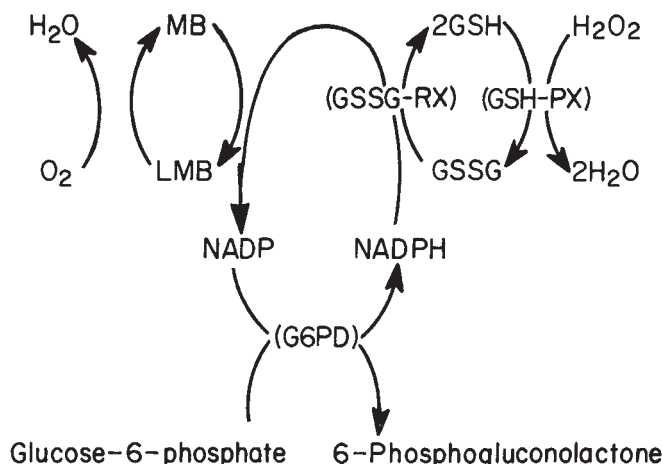


Fig. 1 HMP shunt is regulated by the availability of nicotinamide adenine dinucleotide phosphate (NADP) for the oxidation of glucose-6-phosphate to 6-phosphogluconate in the presence of G6PD. Any reaction that increases dihydronicotinamide adenine dinucleotide phosphate (NADPH) oxidation increases NADP availability and enhances the HMP shunt. Methylene blue (MB) transfers electrons from NADPH to molecular oxygen, forming leukomethylene blue (LMB) as an intermediate. Hydrogen peroxide (formed by interaction of ascorbate and oxyhaemoglobin) sequentially activates glutathione peroxidase (GSH-Px) and glutathione reductase (GSSG-Rx) resulting in NADPH oxidation. During this process, glutathione (GSH) is converted to oxidized glutathione (GSSG) and subsequently is reduced again to GSH.

GSH after RBCs were incubated for 2 h with 20 mM ascorbate.

When erythrocytes were incubated with either 10 or 50 mM cyanate, there was a significant decrease in GSH compared with appropriately treated controls (Table 1a). Unlike the decrease in GSH seen during an oxidant stress⁸, the CNO-induced decrease in GSH was not associated with an increase in GSSG. Stark has previously demonstrated that CNO also can react reversibly with free sulphhydryl (-SH) groups of cysteine and glutathione, forming S-carbamylcysteine and S-carbamylglutathione respectively⁹. Our results are consistent with the formation of S-carbamylglutathione in the intact erythrocyte.

Erythrocyte G6PD also was depressed in CNO-treated cells (Table 1a). Corresponding to this enzyme inactivation, there was a concentration and time-dependent depression of the resting and stimulated HMP shunt (Table 1b). Erythrocyte G6PD from a black patient with A- variant G6PD deficiency containing 1.3 EU/g Hb was decreased approximately 50% to 0.6 EU/g Hb after 2 h incubation with 50 mM CNO (normal adult G6PD in our laboratory is 4.0–7.2 enzyme units per g haemoglobin).

When washed three times, CNO-treated RBCs with decreased GSH were reincubated with or without glucose for 1 h; GSH was regenerated to normal concentrations (Table 1c, T2, T3), which again suggested reversible S-carbamylation of glutathione in erythrocytes. G6PD has at least one reactive thiol group related to the functional integrity of the enzyme¹⁰, and so we tried to establish whether G6PD inhibition was related to S-carbamylation, and therefore reversible. Sluyterman has demonstrated that CNO reacts with the -SH group of papain, resulting in enzyme inhibition, reversible when the CNO enzyme complex was diluted¹¹. When CNO-induced G6PD-deficient erythrocytes were washed to remove unbound CNO, and then reincubated for a further hour to regenerate GSH, no reversal of enzyme inhibition was observed. Further incubation for an additional 2 h failed to remove the inhibition. This irreversibility of G6PD inhibition was consistent with amino, but not thiol, carbamylation of the enzyme. Kinetic analysis of the reaction failed to reveal any change in the Michaelis constants (K_m) for either NADP or G6PD.

Table 1

a, Effect of Cyanate on Erythrocyte GSH, GSSG and G6PD Activity

	GSH μmol/ml. RBC	GSSG μmol/ml. RBC	G6PD EU/g Hb
Control	2.28 ± 0.29 (29)	0.101 ± 0.049 (6)	5.14 ± 0.66 (5)
10 mM CNO × 1 h	1.18 ± 0.38 (7)	0.095 ± 0.047 (4)	4.11 ± 0.49 (4)
50 mM CNO × 1 h	1.33 ± 0.34 (10)	0.115 ± 0.032 (3)	3.05 ± 0.64 (4)
50 mM CNO × 2 h	1.11 ± 0.35 (8)	0.118 ± 0.046 (6)	1.98 ± 0.25 (5)
50 mM CNO × 4 h	1.41 ± 0.25 (8)	—	1.11 ± 0.34 (4)

b, Hexosemonophosphate Shunt Activity: Relative Rate Glucose-1-¹⁴C Oxidation

	Resting	Ascorbate	Methylene blue
Control	1.0	7.70 ± 0.87 (3)	26.8 ± 4.4 (6)
10 mM CNO × 1 h	0.68 ± 0.11 (13)	6.11 ± 0.78 (3)	20.2 ± 3.8 (3)
50 mM CNO × 1 h	0.73 ± 0.10 (3)	3.80 ± 0.98 (3)	15.1 ± 4.2 (3)
50 mM CNO × 2 h	0.62 ± 0.12 (4)	2.33 ± 1.44 (3)	4.0 ± 1.4 (3)
50 mM CNO × 4 h	0.50 ± 0.20 (4)	0.43 ± 0.14 (3)	1.0 ± 0.2 (3)

c, Glutathione Regeneration and Stability in Erythrocytes pretreated with Cyanate

	T2	T3	T5
Control	2.32 ± 0.22 (7)	2.26 ± 0.29 (7)	2.15 ± 0.29 (7)
50 mM CNO	0.99 ± 0.21 (7)	2.08 ± 0.38 (7)	1.06 ± 0.34 (7)

a, Red blood cells were incubated with 0, 10 or 50 mM CNO as indicated, washed three times, and aliquots removed for GSH, GSSG and G6PD assay.

b, Red blood cell suspensions were preincubated as described above. Glucose-1-¹⁴C was added and ¹⁴CO₂ collected and quantified after 2 h further incubation.

c, Red blood cells were incubated with 0 or 50 mM CNO for 2 h, washed three times, and aliquot removed for GSH determination (T2). Erythrocytes were reincubated for a further hour after washing, and GSH determined (T3). Sodium ascorbate (20 mM) was added to both control and CNO-treated cells with regenerated GSH, allowed to incubate for 2 h, and final GSH determinations were made (T5). Numbers in parentheses are the number of experiments.

The HMP shunt provides NADPH for the enzyme glutathione reductase that maintains GSH in a reduced state. GSH is required to protect intracellular and membrane proteins from oxidant drugs¹². Erythrocytes from individuals with G6PD deficiency are unable to maintain GSH levels when they are presented with an oxidant stress⁸. Similarly, when CNO-induced G6PD-deficient RBCs with regenerated normal levels of GSH were stressed with ascorbate, their GSH was unstable (Table 1c, T3, T5).

In view of the reactivity of cyanate for amino and thiol groups of proteins, it is necessary to determine which carbamylated proteins are functionally altered *in vitro* and *in vivo*. We are uncertain whether our *in vitro* observations will limit the clinical usefulness of cyanate for the treatment of sickle cell disease, particularly among the 11% of Negroes with sickle cell disease who are also deficient for G6PD¹³. This enzyme defect is due to a structural alteration of the protein, resulting in accelerated intracellular decay of enzymatic activity¹⁴. It will be important therefore to determine if the intracellular stability of this G6PD variant is altered during cyanate administration *in vivo*.

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B. E. GLADER
M. E. CONRAD

Department of the Army,
Walter Reed Army Institute of Research,
Walter Reed Army Medical Center,
Washington DC 20012

Received February 14; revised March 29, 1972.

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Possible Role of 18-Hydroxy-deoxycorticosterone in Hypertension

OUR studies on two strains of rats with opposite genetic predisposition to hypertension showed that their adrenal steroid production differed significantly^{1,2}. This suggested a theory concerning the role of mineralocorticoids in the initiation of hypertension. The two strains of rats³⁻⁹ were separated genetically on the basis of their disparate blood pressure responses to salt (NaCl). The members of one strain (sensitive or S) rapidly develop fatal hypertension from salt intakes to which members of the other strain (resistant or R) respond only mildly, if at all. These same effects were shown when five other techniques were used to induce experimental hypertension (deoxycorticosterone acetate plus NaCl; unilateral renal artery compression without NaCl; cortisone without NaCl; adrenal regeneration with NaCl; uninephrectomy without (or with—unpublished observations) NaCl). Since the genetic influence seems critical in determining the effectiveness of a variety of stimuli known to induce experimental hypertension, we have suggested that a similar situation prevails in human hypertension^{6,8}. Breeding experiments have shown that the control of these dramatic blood pressure differences between S and R rats is due to the expressions of two to four genes⁹. Our objective was to identify the biochemical mechanism controlled by each gene.

One such mechanism involves the adrenal mineralocorticoid 18-hydroxy-deoxycorticosterone (18OH-DOC). S rats have significantly higher 18OH-DOC levels in their adrenal venous blood (Table 1) and peripheral plasma (Table 2) than R rats. Deoxycorticosterone (DOC) and aldosterone secretion, by contrast, are identical in the two strains and corticosterone (B) secretion varies inversely to 18OH-DOC between strains (ref. 1 and Table 1). Like B and DOC, 18OH-DOC is secreted

Table 1 Steroid Secretion in Adrenal Venous Blood of Salt Resistant (R) and Salt Susceptible (S) Rats maintained on Low Salt (0.4% NaCl) Diet

Steroid μg/rat/h ± s.e.	R	S	t test significance level
Aldosterone	1.08 ± 0.04	1.06 ± 0.15	NS
DOC	2.33 ± 0.44	2.34 ± 0.33	NS
18OH-DOC	26.3 ± 4.1	55.7 ± 9.2	0.01–0.025
B	160.5 ± 10.3	116.6 ± 9.0	0.005–0.01

Adrenal venous blood from the left adrenal was collected for 1 h from each of twelve R female and twelve S female rats under urethane anaesthesia. Blood loss was replaced by 0.9% saline intravenously. Blood from two rats was pooled per sample, and there were therefore six independent samples for R and six independent samples for S rats. Masses of the various steroids were determined in each sample by gas chromatographic methods¹ and the steroid data represent μg/two left rat adrenals/h which essentially equals μg/rat/h. The means ± s.e. are given for the six independent samples from each strain. A logarithmic transformation was used in calculating the t test. NS, not significant, that is, $P > 0.05$; DOC, deoxycorticosterone; 18OH-DOC, 18-hydroxy-deoxycorticosterone; B, corticosterone.

Table 2 Results of 2 × 2 Factorial Analysis of Variance on Peripheral Plasma Levels of 18OH-DOC in Salt Resistant (R) and Salt Susceptible (S) Rats fed High (8%) or Low (0.4%) NaCl Diets

18OH-DOC μg/100 ml.	Resistant (R)		Susceptible (S)		Significance levels Salt Strain diet	Inter- action
	Low NaCl	High NaCl	Low NaCl	High NaCl		
	1.20 ± 0.19	1.90 ± 0.41	3.54 ± 0.63	3.08 ± 0.69	> 0.005 NS	NS

There were twenty-four female rats in each of the four treatment categories (total of ninety-six rats in the experiment). The peripheral neck blood from three rats in the same treatment category was pooled following rapid decapitation. There were, therefore, eight independent samples for each treatment category. 18-Hydroxy-deoxycorticosterone (18OH-DOC) was measured in each sample by gas-liquid chromatography. The significance levels are from a 2 × 2 factorial analysis of variance which was done on logarithmically transformed data. NS, not significant, that is, $P > 0.05$. The untransformed means ± s.e. are given for the eight samples in each treatment category. All rats had been weaned to low salt diet, and 2 weeks after weaning, high salt diet was started where indicated. All animals were killed at 16 weeks of age.

Table 3 Acute Response of Aldosterone Secretion in Adrenal Venous Blood of Salt Resistant (R) and Salt Susceptible (S) Rats to Drinking 1% Saline

μg/rat/h ± s.e.	Days on 1% saline				Significance levels Strain Saline	Inter- action
	0	1	2	4		
S	1.074 ± 0.106	0.834 ± 0.097	0.518 ± 0.156	0.357 ± 0.117	0.696	
R	1.055* ± 0.019	0.616 ± 0.066	0.541 ± 0.139	0.537 ± 0.156	0.687	NS < 0.005 NS
$\bar{x}$	1.064	0.725	0.530	0.447		

* One inordinately high value (probably contaminated with standard aldosterone) was discarded from this mean. Inclusion of this value, however, did not change the conclusions or significance levels presented.

Adrenal venous blood from the left adrenal was collected for 1 h under urethane anaesthesia from six female S and six female R rats on each of the 4 days indicated in the table (total of twenty-four S and twenty-four R rats). Blood loss was replaced by 0.9% saline intravenously. Blood from two rats in the same treatment category was pooled per sample to yield three independent samples in each treatment category. Aldosterone was measured by glass liquid chromatography¹ on each sample and the data represent μg/two left rat adrenals/h which essentially equals μg/rat/h. The significance levels are from a 2 × 4 factorial analysis of variance done on logarithmically transformed data. NS, not significant; that is, $P > 0.05$. The untransformed mean of the three independent samples ± s.e. is given for each treatment category.

by the inner zones of the rat adrenal cortex¹⁰, and is responsive to ACTH¹¹. We were not surprised to find that 18OH-DOC is not affected by salt loading (refs. 1 and 2 and Table 2). This was in contrast to aldosterone, which was rapidly reduced to the same extent in both strains (Table 3).

The total output of adrenal mineralocorticoid (MC) activity in the rat is probably well represented by the sum actions of aldosterone, deoxycorticosterone (DOC), 18OH-DOC and corticosterone (B). MC activity was evaluated in terms of sodium (Na) retaining ability and not in terms of the urinary sodium/potassium (Na/K) ratio. Conditions which favour sodium retention are thought to favour the development of hypertension, but the effects of various steroids on Na or K excretion vary independently. Thus, even though the urinary Na/K ratio is a useful bioassay parameter, its use here is inappropriate since we want to evaluate as accurately as possible the conditions which act on sodium retention. Corticosterone, for example, has a rather significant effect on the Na/K ratio which is due mostly to a large effect on K excretion; the Na retaining effect of corticosterone is only 1/200 that of DOC^{12,13}. Aldosterone MC potency measured by urinary Na/K ratio is 100 times that of DOC, but this is due in

part to the stronger effect of aldosterone on K excretion. In terms of Na retention, aldosterone is only twenty-five times as potent as DOC. The Na retaining potency of 18OH-DOC has been variously reported to be between one-twentieth and one-tenth that of DOC¹⁴ and to be equal to that of DOC¹⁵. The different estimates probably reflect different assay conditions (salt and water loads). In the discussion to follow, the Na retaining potency of 18OH-DOC is taken to be one-tenth that of DOC which is a conservative estimate in light of the higher values reported.

Approximate figures for the relative MC potencies of aldosterone : DOC : 18OH-DOC : B in terms of Na retaining ability are 25 : 1 : 0.1 : 0.005 (refs. 12–15). Multiplying these relative potencies by the respective secretion rates in Table 1 gives an estimate of the relative MC activity contributed by each steroid. In rats on a low salt intake, the inner zone MC steroids (DOC + 18OH-DOC + B) make up about 17–24% of the total MC activity depending on the genotype (R or S). Since aldosterone secretion is sensitive to variations in salt intake, as sodium intake rises and aldosterone is correspondingly suppressed, the fraction of total MC activity made up of steroids unresponsive to salt (DOC + 18OH-DOC + B) will be increased. Fig. 1 gives a diagrammatic representation of this.

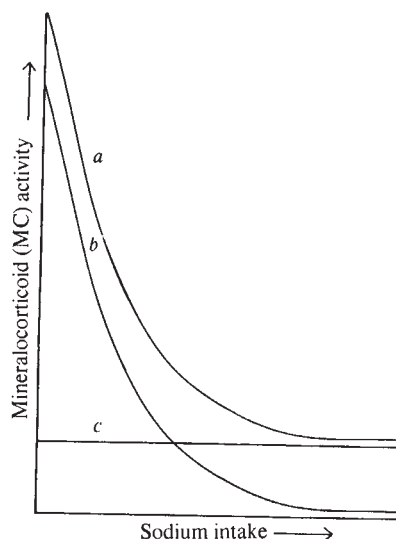


Fig. 1 Theoretical representation of changes in mineralocorticoid (MC) activity with sodium intake. The hyperbolic character of the aldosterone response to sodium (*b*) is based on the data of Laragh *et al.*¹⁶. The constant character of MC activity of inner cortical zone steroids (*c*) represents the concept that 18OH-DOC, B and DOC are largely unresponsive to salt^{1,2}. The total MC activity curve (*a*) is the sum of the other two curves. The representation of MC activity of inner cortical zone steroids at about 15% of the total MC activity on low salt intake (left side of the figure) is based on assumptions and calculations given in the text.

To cause experimental hypertension, both aldosterone and DOC either require excess salt or their hypertensinogenic potencies are significantly increased by salt¹⁷. This may well hold true for 18OH-DOC as well. Beneficial effects of low salt intakes on hypertension, which emerge in spite of the enhancement of aldosterone secretion, may be explained by this interrelationship. It has been difficult, however, to explain the harmful effects of salt excess which markedly depresses aldosterone secretion. This discrepancy may be reconciled by the persistent secretion of MC steroids in significant quantities, in spite of high salt intake. Since further sodium intake cannot be compensated for by further decrease in total MC activity, the total MC activity is inappropriately high for the sodium load of the animal. This situation (shown on the right side of Fig. 1) is conducive to sodium retention and hypertension. Our animals that are genetically predisposed to secreting increased 18OH-DOC (and to developing hypertension) would clearly be at a disadvantage in suppressing total MC activity on high salt intake. This may partly explain the interaction of salt and strain on

blood pressure. The secretion of total MC activity in arbitrary units can be found for each strain by multiplying the steroid secretion rates in Table 1 by the respective MC potencies and adding the numbers obtained for each strain. On low salt, $S=35.0$ and $R=32.8$ in arbitrary units and the ratio of total MC activity secretion $S/R=35.0/32.8=1.06$. In the case of high salt intake, if aldosterone secretion could be completely suppressed, the same calculations show the ratio of total MC activity secretion would be $S/R=8.5/5.8=1.47$. This demonstrates the theoretical way in which suppression of aldosterone by salt could affect the relative importance of the inner zone MC activity between strains.

Unpublished observations indicate that the differences between S and R rats in 18OH-DOC secretion and the concomitant reciprocal difference in B secretion (Table 1) result from the action of a single mendelian gene inherited by co-dominance. At the present time, 18OH-DOC is unavailable in quantities sufficient for chronic injection studies in animals. The fact, however, that the gene controlling 18OH-DOC was segregated while selectively inbreeding for hypertension must suggest that this compound participates in the pathogenesis of hypertension. Melby *et al.*¹⁸ have just reported, furthermore, that about 30% of their patients with essential hypertension secreted excesses of 18OH-DOC. It is reasonable, therefore, to propose that our genetic model may be applicable to man although it must be emphasized that genetic control of 18OH-DOC represents only one of the several genes estimated on the animal model⁹.

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JOHN P. RAPP

Penrose Research Laboratory,
Philadelphia Zoological Society,
34th Street and Girard Avenue,
Philadelphia, Pennsylvania 19104, and
Pathology Department,
University of Pennsylvania,
Medical School,
Philadelphia, Pennsylvania

LEWIS K. DAHL

Medical Department,
Brookhaven National Laboratory,
Upton, New York 11973

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Chromosomal Localization of Human Haemoglobin Structural Genes

WE have hybridized radioactive RNA specific for the production of haemoglobin with human metaphase chromosomes. The regions on the chromosomes which may contain the corresponding structural genes have been identified by autoradiography.

Until now the assignment of various genes to specific human chromosomes has been approached indirectly, by the use of marker chromosomes, deletion mapping and cell hybridization. This has made it possible to speculate on the locus of a few random genes on human chromosomes¹⁻³, where events have been favourable to such studies. But we now have a method by which gene loci can be assigned to specific areas of human chromosomes provided only that it is possible to purify mRNA specific for the locus sought.

Messenger RNA (mRNA) is known to arise from chromosomal DNA⁴, and complementary RNA, such as mRNA, has been shown to hybridize with DNA at the cytological level in numerous organisms⁵⁻⁹. The availability of isolated protein-specific mRNA¹⁰ for these hybridization techniques now makes it possible to determine the chromosomal localization of the genes for these specific proteins.

Specific staining and spectrophotometric methods for the detection of RNA in RNA-DNA hybrids have not yet been demonstrated, so autoradiographic detection is necessary to localize such regions of hybridization within the DNA of a particular chromosome. For this reason, heavily labelled protein-specific mRNA was required. Rabbits were chosen as the source of mRNA both for the ease of preparation of highly specific radioactive mRNA, and for the high coincidence of nucleotide sequences between rabbit and human globin chains¹¹⁻¹³.

Radioactive mRNA was isolated from rabbit reticulocytes by standard procedures¹⁰. Although reticulocytes also contain a little non-globin mRNA, the sharpness of the mRNA peak (Fig. 1), and the fact that these contaminants could only be present as a small percentage of the mRNA population, makes it unlikely that they were significant.

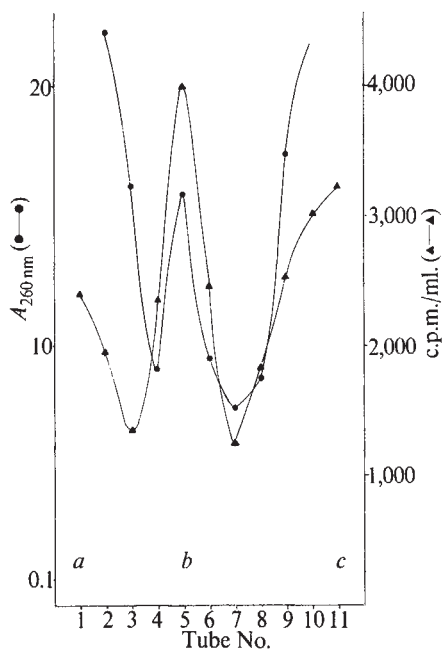


Fig. 1 A_{260} curve plotted from sucrose gradient fractions, with corresponding c.p.m. *a*, Fractions containing ribosomes, polyribosomes, and debris; *b*, fractions representing mRNA; *c*, fractions representing tRNA and puromycin. Methodology after Blobel¹⁰.

mRNA specific for haemoglobin was made radioactive by the injection of ³H-uridine (5 mCi) into phenylhydrazinized rabbits by the method of Burney *et al.*¹⁴. The rabbits were killed and the reticulocytes collected into a heparinized syringe. The cells were washed and lysed as previously described¹⁵. The isolation of globin-specific mRNA was performed by the method introduced by Blobel¹⁰. The reticulocyte lysate (6 ml.) was layered onto a 4-ml. stepwise gradient consisting of 2 ml. each of 2.0 M and 1.35 M sucrose in Tris buffer (50 mM Tris Cl, pH 7.5, 25 mM KCl, 5 mM MgCl₂) and centrifuged for 24 h at 40,000 r.p.m. in a Sorvall 50 rotor. The polyribosome pellet was resuspended in water to a concentration of 200 A_{260} units per ml. and incubated, after the addition of 0.25 ml. of $\times 2$ (50 mM Tris Cl, pH 7.5, 500 mM KCl, 5 mM MgCl₂: TKM) and 0.05 ml. of 0.01 M puromycin per 0.2 ml. polyribosomes, first for 15 min at 0° C, and then for 10 min at

Table 1 Significance of Radioactive Labelling of Chromosomes

Chromosome No.	mm labelled	No. times mm labelled	Probability (%)
2	8-9	1	> 99.0
2	9-10	2	25.8
2	10-11	2	25.8
2	11-12	4	0.54
2	12-13	4	0.54
2	13-14	5	0.051
2	14-15	3	4.6
2	15-16	1	> 99.0
2	16-17	1	> 99.0
2	17-18	1	> 99.0
2	18-19	1	> 99.0
2	19-20	1	> 99.0
2	20-21	1	> 99.0
B	5-6	1	> 99.0
B	6-7	6	0.0032
B	7-8	7	0.00016
B	8-9	6	0.0032
B	9-10	8	0.0000068
B	10-11	6	0.0032
B	11-12	6	0.0032
B	12-13	6	0.0032
B	13-14	5	0.051
B	14-15	3	4.6
6	4-5	1	> 99.0
6	5-6	1	> 99.0
6	6-7	1	> 99.0
6	7-8	1	> 99.0
6	8-9	1	> 99.0
6	9-10	1	> 99.0
6	10-11	1	> 99.0
8	4-5	1	> 99.0
8	5-6	1	> 99.0
8	6-7	1	> 99.0
X	8-9	1	> 99.0
X	9-10	1	> 99.0
X	10-11	1	> 99.0
X	11-12	1	> 99.0
X	12-13	1	> 99.0

All No. 2 chromosomes were adjusted to 21 mm; B adjusted to 16 mm; No. 6 adjusted to 14 mm; No. 8 adjusted to 11.5 mm; X adjusted to 13 mm; total mm/karyotype (forty-six chromosomes) = 500 mm (all adjustments based on average chromosome (length in karyotypes). All measurements taken from top of chromosome; only mm which have label present are considered in this table.

Probability that label in a specific mm is random computed from the following (*Documenta Geigy*, seventh edition, 189, formula 805b): probability of labelling any mm of chromosome of the eighteen karyotypes = 1.0 (100%).

Probability of labelling any labelled mm =

$$\frac{d!}{(d-e)!e!} \cdot \frac{(a-d)!}{a!} \cdot \frac{b!}{(b-e)!} \cdot \frac{c!}{(c-d+e)!}$$

where a = total mm remaining = 9,000 - 1 = 8,999; b = total mm remaining that are identical to one labelled = 36 - 1 = 35; c = total other mm remaining = 8,964; d = total remaining mm to label = 96 - 1 = 95 (96 = total mm labelled); e = frequency of labelling any labelled mm (so that if a mm is labelled five times, $e = 4$). Only four or more hits of any particular mm are being considered significant, because this is significant at the 0.01 level.

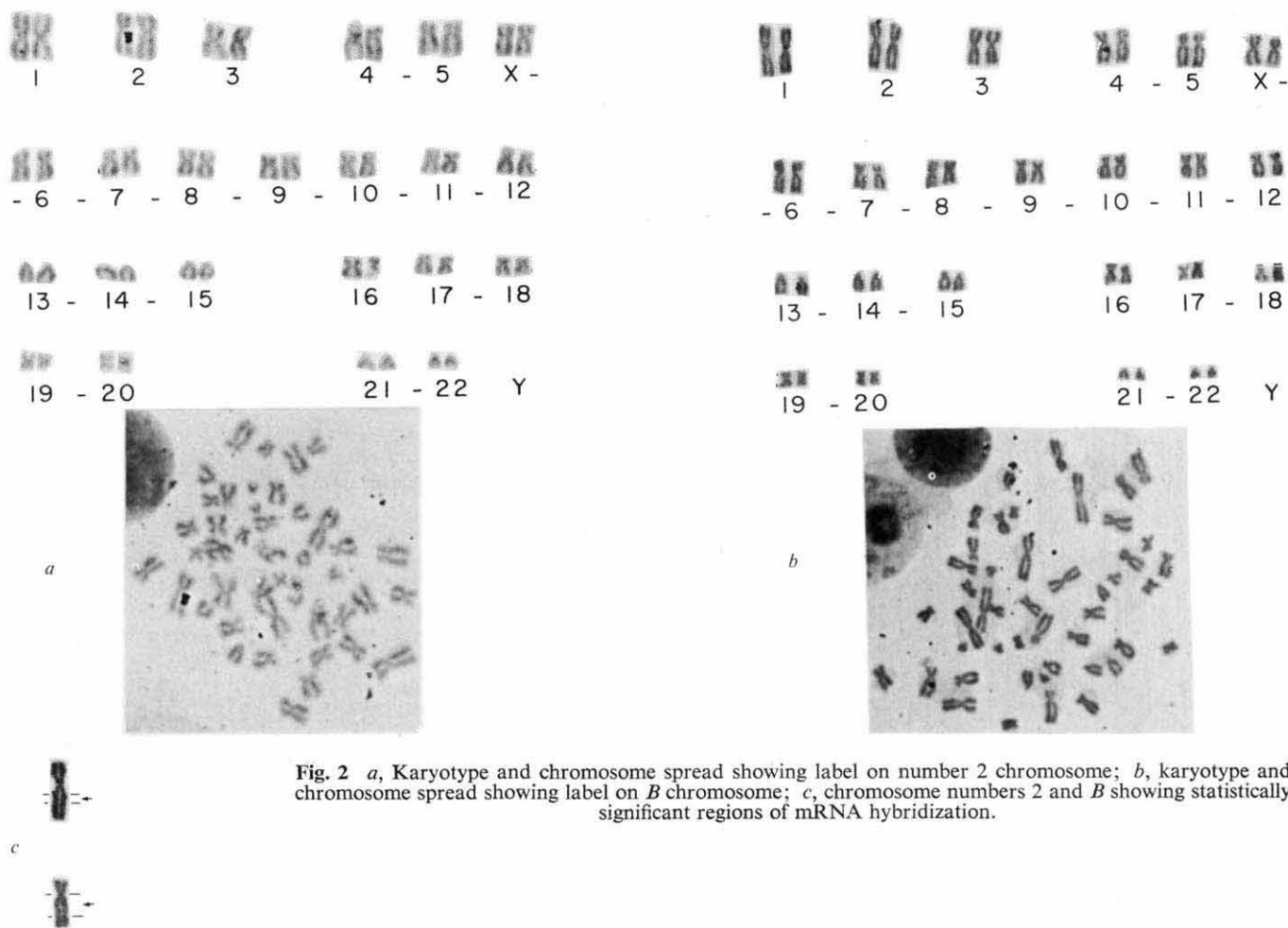


Fig. 2 *a*, Karyotype and chromosome spread showing label on number 2 chromosome; *b*, karyotype and chromosome spread showing label on *B* chromosome; *c*, chromosome numbers 2 and *B* showing statistically significant regions of mRNA hybridization.

37° C. The resulting suspension (0.5 ml.) was layered onto a linear 5–20% sucrose gradient in TKM and centrifuged for 5 h at 37,500 r.p.m. in a Sorvall SW 40 rotor. The tube was then punctured and the A_{260} curve was plotted (Fig. 1). The mRNA peak was collected and frozen at –20° C until needed.

Human metaphase chromosomes were prepared from long term lymphoid cultures¹⁶ by the method of Hirschhorn¹⁷ on glass slides. Each chromosome slide was initially incubated for 10 min at 100° C and plunged immediately into ice. mRNA (0.05 ml.), previously adjusted to 1 M NaClO₄ (ref. 18) by the addition of 5 M NaClO₄, was placed on each slide. A siliconized coverglass was placed on top and sealed to the slide with a rubber compound. The slide was incubated first at 100° C for 10 min, then at 65° C for 20 min, after which the slide was immediately plunged into ice water. The method of hybridization of Pardue and Gall⁸, using $\times 2$ standard sodium citrate (SSC), was also used in some of the experiments and produced identical results. The coverglasses were removed and the slides were stained with 2% aceto-orcein. Autoradiography was performed using Kodak AR-10 stripping film, and after 2 weeks of incubation the slides were developed and photographed.

More than 1,000 metaphases were examined and, of these, eighteen cells were both of the quality to be karyotyped and had label (as mRNA) present on any chromosome. This low frequency is felt to be due primarily to the low specific activity of the mRNA (approximately six disintegrations per 2 weeks per 10⁷ molecules, based on a molecular weight of 200,000^{20,21}).

Statistical analysis of our results (Table 1) demonstrates that there are two areas in the human karyotype which are significantly labelled. Chromosome 2 displays a region from the 11th to the 14th mm (Fig. 2), and a *B* group chromosome (4 or 5) from the 6th to the 14th mm (Fig. 2 and Table 1).

It is thus highly probable that a *B* group chromosome and chromosome 2 are involved in human haemoglobin production. Human β , γ and δ -globin but not the α -chain loci are linked^{21,22} and the chains possess striking linear similarity^{13,23,24}. It is therefore likely, from the relative lengths of the labelled regions of chromosomes labelled with statistical significance (8 mm labelled heavily and consecutively on the *B* group against 3 mm on No. 2), that the β , γ and δ -globin loci are located on the *B* group chromosome, whereas the α -globin gene is likely to be located on chromosome 2. The rare event of a large *C* group chromosome (6–12 and *X*) being labelled (Table 1) is statistically random, and is caused probably either by the hybridization of common nucleotide sequences or by the presence of contaminating mRNA species. Even if all the labelled *C* group chromosomes are considered as belonging to one pair only (because of the difficulty of identifying the members of this group by standard staining techniques), the maximum number of hits in any mm is 2, which is not significant (random $P=25.8$). Other chromosomes were rarely labelled.

Until now, the localization of gene loci has depended on two procedures used together or separately. First, loci were grouped through being together on the same chromosome and showing linkage (non-independence) with one another. Second, similar non-independence was sought between a locus and some recognizable feature of a particular chromosome²⁵ (that is, a chromosome marker). The application of methods in this report provides a third procedure for gene localization. Thus, chromosomal localization of any structural gene is feasible, once its specific mRNA can be isolated.

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PETER M. PRICE
JAMES H. CONOVER
KURT HIRSCHHORN

Division of Human Genetics,
Department of Pediatrics,
Mount Sinai School of Medicine
of the City University of New York,
New York, New York 10029

Received February 29, 1972.

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Light-evoked Impulses from Extra-ocular Photoreceptors in the Squid *Todarodes*

A VARIETY of cephalopods, both octopods and decapods (squids), have extra-ocular photoreceptors, as structural, biochemical¹⁻⁴ and electrophysiological studies⁵ have shown. In the octopods these receptors lie in the mantle cavity on each stellate ganglion, and in the squids they lie, again bilaterally, underneath the cranial cartilaginous casing on and below the optic tract³. Although we recorded generator potentials intracellularly from the receptors in the octopus, *Eledone* and *Octopus*, we found repetitive firing rarely during steady state illumination. And recently in the squid *Todarodes* we have been able to impale cells with micropipette electrodes, but observed only a generator potential; thus we did not establish whether or not impulses are evoked by light and propagated

to the central nervous system. We therefore tried recording directly from the associated nerve fibres since impalement may cause depolarization and thus block the repetitively firing region of the neurone. These studies have shown that propagated action potentials do arise from the photoreceptor cell body on illumination and are conducted down the axons to the central nervous system. This new fact indicates further that this system of photoreceptors may play a significant physiological role in cephalopods.

In the case of octopods the nerve bundle from the epistellar body is inaccessible since it virtually disappears in the neuropile of the ganglion. On the other hand, in the squids such as *Todarodes sagittatus* and *Illex coindetii* a distinct bundle of axons is seen extending from the ventral photosensitive vesicles (known until now as ventral parolfactory vesicles) to the peduncle lobe, both on the right and left side of the optic tract. The squid preparation provides therefore an excellent opportunity to examine the nerve fibres for light-evoked impulses.

Specimens of *Todarodes* with a mantle length of 25 to 40 cm were caught in the waters off Ischia, an island 20 miles from Naples. Experiments were conducted only with animals that were freshly caught and alive just before dissection since we found that excitability deteriorated rapidly within 1 to 2 h of death. After decapitation the brain was exposed and both sets of ventral vesicles were removed, each with the attached nerve which was severed at the base of the peduncle lobe. Preparations were mounted in a suitable chamber containing seawater at 20°–25° C. The vesicle-end was pinned to the cork-covered base of the chamber and the nerve, 6–8 mm long, was dissected into small bundles. Each bundle was suspended in air by means of a wick electrode attached to a manipulator, a second electrode in the bath serving as the indifferent electrode. An electro-magnetically activated shutter in conjunction with a tungsten light source and a fibre optics light pipe provided light pulses to excite the receptors in the vesicles.

Fig. 1 is a recording obtained on a nerve bundle in which many fibres were still present, as can be seen by the initial diphasic recording of large amplitude which we interpret as arising from a synchronous excitation of many axons as a consequence of the rapid onset of the generator potential (transient component). Following the synchronous volley, there is random activity during the steady state phase of the generator potential arising from asynchronous excitation of individual fibres. We have not been able to obtain a single-

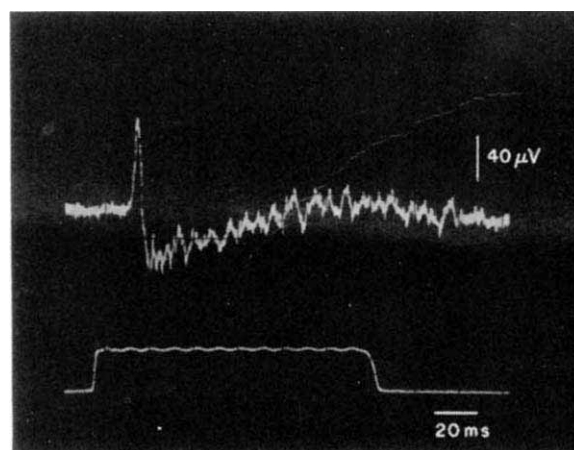


Fig. 1 Light-evoked electrical response of small bundle dissected from the nerve from the ventral photosensitive vesicles. Initial diphasic recording is due to synchronous excitation of nerve fibres with onset of generator potential ("transient component" of generator potential). Asynchronous firing follows in response to steady state component of generator potential. Light pulse in lower tracing was recorded by photoelectric cell.

fibre preparation; this will require considerable refinement in the technique of dissection in view of the small size of the axons (less than 1 μm in diameter⁴). Fig. 2 is a recording of the monophasic compound action potential resulting when the distal segment of the nerve was allowed to dry and became inexcitable. The decreasing latency with increasing light intensity is a reflexion of the corresponding onset of the transient component of the generator potential in the soma of the photoreceptor cell. (Although the inaccessibility of the nerve has not made it feasible to record impulses from the epistellar body, there is no reason to doubt that here, too, impulses are evoked by light.) Our preparations have been viable for as long as 2 h whereas many attempts by various investigators to record responses (*in vitro*) in the retinal nerves on illuminating the retina have yielded negative results; recordings from the retinal nerves have been reported only for *Loligo*⁶ in restrained animals kept alive by perfusing the mantle cavity with oxygenated seawater. It would appear that perhaps the retina is more oxygen-dependent than the extra-ocular system of photoreceptors. Whether or not this difference has physiological significance remains to be explored.

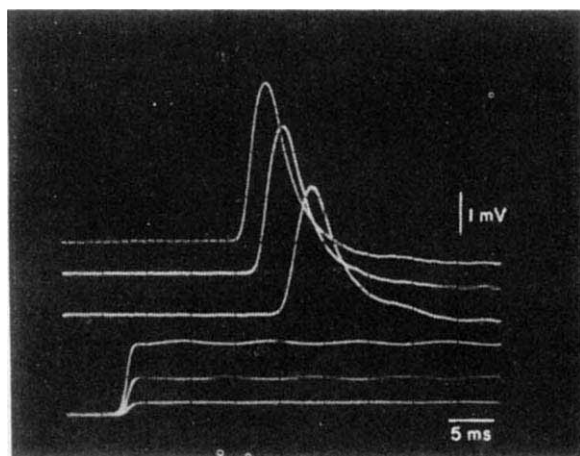


Fig. 2 Monophasic recording obtained when distal segment of nerve bundle was allowed to dry and thus became inexcitable. Three upper recordings given by the lower, middle and upper tracing show response to low, medium and intense light pulse, respectively. Baseline displaced upward manually between successive recordings. Corresponding light stimulus registered in lower set of recordings.

Recently several more genera of deep-sea squids—in addition to *Liocranchia*²—have been discovered with extra-ocular photoreceptors (R. E. Young, manuscript submitted for publication) and indeed in one species as many as four sets of vesicles were found. Thus as more structural and physiological data appear, support for Messenger's conclusion that "in this class of molluscs extra-ocular photoreception is widespread"² increases. It is clear that this new and intriguing system of photoreceptors merits the serious attention of investigators from many disciplines as in the case of the photoreceptors present in the epiphysis (diencephalon) of frogs^{7,8}, lizards⁹ and fishes¹⁰⁻¹⁴.

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ALEXANDER MAURO*
OVE STEN-KNUDSEN†

Zoological Station,
Naples (Ischia)

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* Permanent address: Rockefeller University, New York.

† Permanent address: Department of Biophysics, University of Copenhagen, Denmark.

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Testosterone, Search Behaviour and Persistence

WE have found that testosterone markedly increases the persistence with which male chicks respond to a particular type of stimulus. The data reported here are from search tests, but the effect has now been confirmed in discrimination and open-field tests. Apparently similar effects of androgens on cognitive processes in men have recently been described^{1,2}, so that it is possible that we are dealing with an effect which is present in both birds and mammals.

At test, two kinds of food grains are distributed over the test floor, and it is arranged by earlier training that the searching animal, although preferring one type, is quite prepared to take either. In such circumstances a large number of targets are available for each peck, and the ability to take one type of food in long runs (as normal chicks usually do when the preferred food is easy to detect at a distance) is probably one aspect of a more general ability to avoid switching attention to distracting stimuli.

Two types of grains of chick starter food which differed only in appearance (one being dyed red and the other remaining yellow) were used in these tests. One or other of the same two types of food was available at all times (except before and during test), scattered over the floor of the home cage. At test, equal quantities of both types of food were presented randomly but evenly distributed over a standard test floor (7–8 grains/inch²). In "pebble" tests, floors were covered with firmly glued-down pebbles of the same size (1–2 mm diameter, 8/inch²) and colour as the type of food which was known to be preferred. These formed a difficult background over which to search. Pebble tests were preceded by one or more brief training sessions in which birds rapidly learned to avoid pecks at pebbles. In "clumped" tests, food grains (and pebbles, if used) were arranged in 2 inch by 2 inch square clumps, each of which contained five red and five yellow food grains. Clumps were separated by one inch strips of bare floor, so that one or two steps took the chick from one clump to the next. Even so, the clumps were treated to some extent as discrete areas of search. The first 100 pecks in each test are considered; preferred food was always available at some point on the test floor throughout this period, although locally it was repeatedly exhausted. Choices were recorded manually on an event recorder. The observer's eye was at floor level and only a few inches from the chick's bill; as a result the target of virtually all pecks could be recorded. Birds were housed individually and tested either in the home cage or in a chamber to which they were fully accustomed.

Testosterone was given intramuscularly in oil as a single dose (25 mg except in experiment 2) of the oenanthate whose release continues over more than two weeks³. Birds receiving one of the three dosages used (below) are referred to as Ts. Controls (Cs) received vehicle only. Warren sex-link chicks were used, which were male unless otherwise stated. The

colour of food in the home cage from the second day of life up to the first test day may be summarized by a sequence of letters, one for each day, except where a diagonal indicates a change during a day: experiment 1, RRRRRRRR*; experiment 2, RR*YYYYRR; experiment 3, RRRYYR*RR/Y; experiment 4, RRRY/RR*YRY. The asterisk shows day of injection. Fuller procedural details are given elsewhere⁴.

Increased persistence of choice in Ts is clear in the first experiment (1a–b, Table 1). Birds had marked red preferences, following almost continuous exposure to red, with experience of yellow only in a 5 min training session. They were fed by tube into the crop between injection and testing, so that there was no opportunity for interaction between effects of testosterone and experience with the training food.

Table 1 Duration of Runs on Different Backgrounds

		Before injection		After injection			
		1a Pebble	T	1b Pebble	T	1d Plain	C
Mean run on	P food	11.0	12.5	36.7*	7.7	2.7*	16.5
	NP food	2.1	3.4	0*	3.5	2.5*	1.0
Longest run on	P food	27.5	38.0	69.5*	16.5	7.5*	48.0
	NP food	3.0	7.0	0*	5.0	6.0*	1.0
Mean run on pebbles		1.2	1.4	1.0§	2.0	—	—
	% P food	84.5	80.0	97.5*	76.5	55.0*	95.5
% pebbles		1.5	3.0	2.0†	7.5	—	—

The significances of the differences between the scores of testosterone-treated birds (T, $n=10$) and those of control birds (C, $n=10$) are shown as: *= $P<0.001$; †= $0.01>P>0.001$; ‡= $>P>0.01$; §= $0.05>P>0.02$ (Mann-Whitney, one-tailed). The changes in scores between experiments 1a and 1b were not significant for controls, but are markedly so ($P<0.001$, Wilcoxon, one-tailed) for mean runs on P food and on NP food made by Ts. (One-tailed tests were used, because earlier experiments allowed the changes observed to be predicted. The mean run length was found for each individual since it characterized more fully their pattern of responses than did a median value. Non-parametric tests⁸ were then applied, using the groups of means from each group. Median values for these groups of means are given above.)

Tests using a continuous pebble floor were given just before and 48 h after injection. Cs made runs on both types of food, changing readily from one to the other, although usually with much longer runs on the familiar preferred food. Ts, on the other hand, made extremely long runs on red food, with scarcely any pecks on the yellow. They also avoided pebbles more completely. The rarity of pecks at yellow food in Ts does not result from a greater unwillingness to peck the yellow food in all situations: on a continuous plain floor (experiment 1d, Table 1), Ts showed very short runs on both types of food.

The reason for this marked change becomes clear when the results from a plain clumped floor are examined (experiment 1c, Table 2). Here the pecks on non-preferred (NP) food were found almost invariably to occur immediately before a move to another clump. The first pecks in the new clump were almost invariably on preferred (P) food once again, so that no permanent change in search object is involved. When Cs take enough NP food for their behaviour to be studied in this respect (below), it proves to be quite different, in that once NP food has been pecked, it then continues to be taken in a new clump. Ts have now been shown by independent tests to be more persistent in their choice of places to peck, and there is little doubt that it is this which causes them to continue pecks at a clump after P food is exhausted. It is here argued that NP pecks given by Ts just before a move represent pecks at an object which does not fully match the specifications used in search, rather than a change of such specifications. The difference between search over a pebble, and over a plain background is then explicable, since in the former any pecks delivered at objects which do not fully match search specifications will be at pebbles (which resemble

Table 2 Patterns of Search Within a Single Clump of Food

Type of search: %	Expt. 1c Plain floor (a)		Expt. 1e Pebble floor	
	T	C	T	C
P food only	35*	88	80‡	52
NP food only	10	1	0†	14
P followed by NP	35‡	9	9	12
NP followed by P	4	0	0	4
Mixed	16‡	2	11	18

Search at one clump terminates when a bird transfers its pecks to another (a matter of 2 inches or less: text). A "mixed" pattern of search is one in which the bird changed more than once between the two types of food while feeding continuously from one clump. Data are lumped over 10T and 10C for presentation in the table, but significances (which are indicated as in Table 1) are calculated by comparison of individual scores (Mann-Whitney, one-tailed: results predicted). The much greater frequency of P.NP than NP.P search between moves is significant ($P 0.01>P>0.001$) for Ts in experiment 1c (Wilcoxon, two-tailed).

P food in hue and saturation) rather than at NP food. It is clear that pecks at pebbles are carefully avoided, which implies a rigorous use of search specifications, resulting in the avoidance of NP food as well as pebbles.

Such effects of testosterone have now been found repeatedly. Two further experiments using plain floors are selected for description here. Experiment 2 (Table 3) used birds, most of which had a yellow preference, contrary to experiment 1. Injection was on day 3 (at three different dosages: below) and thus preceded training; in experiment 3 it followed training without markedly changing the results. Birds had extensive experience with both types of food in the home cage before testing, so that differences in responsiveness to novel food could not have been involved. High dose males showed mainly single pecks at NP food, which, as has already been argued, do not represent shifts to search for NP food, whereas Cs gave definite shifts resulting in short runs (Table 3).

In experiment 3 the effects of exposure to one type of food immediately before testing ("priming") was examined. It was run in two halves, separated by 18 h exposure to NP food (yellow, given overnight), which increased the likelihood that NP food would be taken. In each half every bird was primed once with yellow food and once with red. In priming a bird was allowed fifty pecks, and then was moved without handling on to the adjacent clumped plain test floor. In the first half, when the preference for P food was high, Ts were much more resistant to priming than Cs (Table 4), just as they are able to peck NP food during tests without shifting to search for it. After NP priming enough NP pecks appeared in Cs before each move to allow a comparison with Ts: Ts are very much more likely to return to P food after the move, whereas Cs tend to shift, search and continue taking NP food ($0.01>P>0.001$).

All experiments agree that Ts show increased persistence in search for a particular type of food grain and in particular places, despite the presence of alternative food objects and alternative objects of search.

Data for three dosage levels are given in Table 3. In male chicks, the two higher doses were equally effective in producing the change in search behaviour, while the lowest dose was significantly less so. Exactly the same was true of the facilitation of copulation (and of attack and comb growth)^{6,7}. Neither search behaviour nor copulation and attack showed any change due to testosterone at any dosage in female chicks (Table 3); comb growth confirmed that circulating levels of testosterone were comparable in males and females receiving the same dose. The effect of testosterone on persistence may thus depend on central changes similar to (but not necessarily identical to) the changes induced by physiological levels of androgens, which are responsible for the facilitation of copulation and attack in adult males. Such changes must involve receptor sites or mechanisms that are absent in females.

The differences in search between control males and females

Table 3 Dosage Dependency of Persistence Effect

Dosage (mg)	Males				Females			
	25	12.5	5	C	25	12.5	5	C
% P food	86.7	83.9	61.1	74.0	85.3	77.4	92.4	90.2
Mean run on { P food	9.9	6.2	6.5	11.3	10.7	8.5	12.2	10.0
NP food	1.33 ‡	1.20 ‡	2.00 §	3.67	1.67	2.00	1.00	1.00
Mean copulation score	7.13 ‡	7.20 ‡	6.30 §	4.05	1.95	1.50	1.67	1.50

Copulation scores represent intensity of copulation at the same age as that at which the search test was given to the same animals, along a ranking scale from 0 (ignores copulation object) to 11 (full intense copulation on two immediately consecutive tests). Significances of differences from control values are shown as in Table 1 (Mann-Whitney, one-tailed: results predicted). Females show no effect of testosterone on any scores but they do differ significantly from males in both search behaviour and copulation scores; this will be discussed elsewhere. The 5 mg group of males had scores differing significantly ($0.05 > P > 0.01$) from those of the two higher dose groups in the direction of control scores, for both copulation and run on NP food. Group sizes were (from left to right): 10, 10, 10, 9, 10, 7, 10.

Table 4

	Expt. 3a				Expt. 3b			
	Primed with P	Primed with NP	Primed with P	Primed with NP	Primed with P	Primed with NP	Primed with P	Primed with NP
% P food	94	96	89	79 ‡	95	91.5	64 *	75.5 *
Mean { P food	13.7	33.5	13.3	10.2	21.0	21.0	4.8 *	8.6 *
run on { NP food	1.4	2.0	1.6	3.0	1.5	2.5	2.1 *	3.5 ‡
Mean longest run on NP food	2.0	2.0	2.0	4.0 §	2.0	3.5	4.0 ‡	6.0 ‡

Experiment 3a was followed by overnight exposure to NP food (text), so that preference for P food was reduced in experiment 3b. Tests were on plain clustered floors. Birds which would not eat NP food during priming were excluded: as a result 13T and 11C were tested in 3a, and 15T and 14C in 3b, out of 17T and 17C. Significances of differences between P and NP priming are shown as in Table 1 (Wilcoxon, one-tailed; results predicted).

Table 5 Effects of Food Deprivation on Search

Hours of deprivation	Mean run on P food		Mean run on NP food		%P food taken	
	T	C	T	C	T	C
0	4.1 ‡	15.3	3.1	3.0	52.5 ‡	84.0
1	4.5 *	27.4	2.0 ‡	3.2	71.0	81.0
3	4.6 *	26.9	2.1 ‡	1.5	67.0 ‡	94.0
5.0	4.9 *	16.5	2.1	1.5	67.5	89.5

Test floor was plain and continuous. Significances, shown as in Table 1, are given for differences between Ts and Cs. Mann-Whitney tests (one-tailed) were used for runs on P food, and Moses test for runs on NP food, since control scores were bimodal, lying on either side of T scores. (Some controls gave very long runs on NP food and others only occasional pecks.) The only significant effects of deprivation were between 0 and 1 h deprivation in Ts: $0.01 > P > 0.001$ for mean NP run, and for percentage P food.

apparent from Table 3 will be discussed elsewhere⁴. They probably do not depend on high persistence in females.

In experiment 4 plain floor tests were carried out over a range of deprivation levels. Little or no difference existed between the patterns of search of either Ts or Cs at 1, 3 or 5.5 h. There was no suggestion that Ts behaved like Cs which were more (or less) hungry than themselves (Table 5).

Behaviour at zero deprivation (achieved by crop loading with 1 ml. of 0.5 M glucose) was significantly different in Ts, but not in Cs. The difference was slight, however, even though in both Ts and Cs the pecks were no longer feeding pecks but were delivered with closed bill. Thus, even when feeding proper had ceased, the characteristic differences between Ts and Cs remained almost undisturbed.

Broverman and his associates^{1,5} have provided evidence for an effect of androgens in young men which may be similar to that just described for chicks. Subjects with high androgen levels were identified by body hair development, thick-set physique and high 17-ketosteroid excretion, and were found to perform well above average (and significantly so) on tasks requiring sustained rapid volleys of the same responses to a limited set of stimuli. Out of the battery of tests administered,

that which was most obviously of this type (and on which high androgen males performed very well) was the "speed of naming repeated objects", in which three different figures had to be named as quickly as possible in a long sequence, in which they were repeated at random. On the other hand, high androgen males did not differ at all from average in their scores on a second test, which was closely similar in method of administration ("speed of recognition test"). This required the subject to examine the figures, which were of a wide variety of objects and were not repeated, as rapidly as possible, making sure only that they depicted something familiar, until he reached a nonsense figure. The first is comparable with the search tests administered to chicks, and the results are explicable if it is supposed that high androgen men are able to maintain search specifications more persistently over a long series of similar recognitions without interference from distracting stimuli; in this they would resemble male chicks injected with testosterone. Increased persistence of specifications would, however, clearly not help in a test like the second, in which each recognition was of a different pattern. This and other evidence is examined further elsewhere.

The most basic new hypothesis suggested by the changes in search behaviour induced by testosterone is that the degree of persistence in use of central specifications for stimuli is an independent variable of importance. High persistence of this type can be distinguished from inability to inhibit or withhold responses by the fact that testosterone affects neither the ability to maintain widely spaced responses on a DRL operant conditioning schedule (P. Messent, personal communication) nor extinction following non-reinforcement, although it does reduce distractibility (as shown by responses to other irrelevant stimuli) in such situations.

Functionally, it should be noted that the adaptive significance usually assigned to the facilitation of fighting in male vertebrates by androgens, namely to make it possible to obtain such resources as nest or burrow, mate and food for young, could be more economically achieved by an increase in persistence. Even when contest is necessary, victory may be better achieved by persistence in display than by the very ready elicitation of attack at first encounter.

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R. J. ANDREW
L. J. ROGERS

*Ethology and Neurophysiology Group,
Biology School,
University of Sussex,
Brighton*

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Control of Sebum Secretion by the Posterior Pituitary

HYPOPHYSECTOMY decreases sebum secretion in the rat¹⁻³. The anterior pituitary hormones, adrenocorticotrophin (ACTH), thyrotrophin (TSH) and the gonadotrophins increase sebum secretion⁴⁻⁹ through actions on their respective target glands⁶⁻¹⁰. It has hitherto been assumed that sebum secretion is under anterior pituitary control, and the possibility that the posterior lobe may be involved has been completely ignored.

We were able to investigate this possibility when we noticed that some of our hypophysectomized rats failed to lose weight and their adrenals, thyroid glands and testes were not atrophied, yet sebum secretion was decreased. Serial sections of the sella turcica and neighbouring tissues revealed that these were rats in which we had removed the posterior lobe (pars intermedia and pars nervosa) leaving the anterior lobe intact.

We therefore set out to study the possibility that sebum secretion is controlled by the posterior pituitary.

Male Wistar rats between 8 and 9 weeks old were used. Total hypophysectomy was achieved by the conventional para-pharyngeal method and only the posterior lobe was removed through a more posteriorly placed trephine in the base of the skull. At autopsy no trace of pituitary tissue was found in 24 rats. In 9 rats there was a complete removal of the posterior lobe, but microscopic examination showed that anterior lobe tissue was intact and histologically normal. Nineteen operated rats served as intact controls. (Results from sham operated rats are not included because they were no different from the intact controls.)

The rate of sebum secretion at 12-13 weeks was measured by the method of Archibald and Shuster¹¹. All rats were then killed and the thyroid, adrenals, testes, seminal vesicles, prostate and preputial glands were removed and weighed.

After total hypophysectomy, sebum secretion decreased as did body weight, weight of adrenals, testes, seminal vesicles and prostate (Fig. 1 and Table 1). The decrease in sebum secretion has been attributed to the removal of the trophic hormones of the anterior pituitary⁴⁻¹⁰, but this view cannot be the full explanation, for when only the posterior lobe was removed there was little change in anterior pituitary function as indicated by body weight and weight of thyroid, adrenals and testes. Androgen production was sufficient to maintain the weight of the seminal vesicles and prostate (Table 1), yet sebum secretion was almost as low as after total hypophysectomy (Fig. 1).

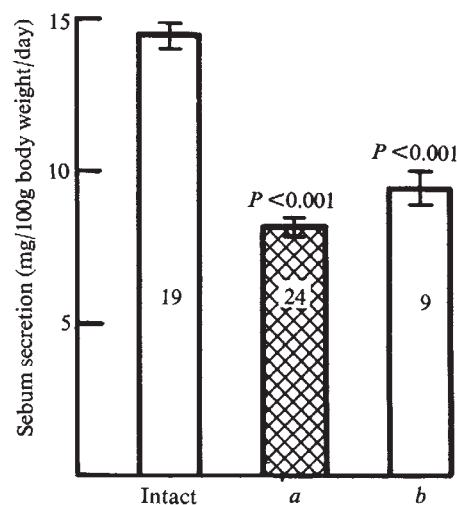


Fig. 1 Effect of total (a) and posterior (b) hypophysectomy on the rate of sebum secretion in the rat (mean $\pm$ s.e.m.). The number of animals in each group are given in the appropriate block. The *P* values refer to the significant difference when compared with control values.

Although continued anterior pituitary function would account for the slightly higher rate of sebum secretion in the posterior hypophysectomized rats than in those with total hypophysectomy, it is obvious that the anterior pituitary alone could not maintain sebum secretion at a normal level. We therefore conclude that a hormone of the posterior lobe is necessary for the maintenance of sebum secretion. The absence of this hormone could account for much of the decrease in sebum secretion after total hypophysectomy. The preputial glands are specialized sebaceous glands and our general conclusion is therefore supported by the further observation that the changes in preputial gland weight showed a similar dependence on the posterior lobe (Fig. 2).

The mode of action of the posterior pituitary will require further study. Because the rate of sebum secretion in rats that have been castrated, adrenalectomized and thyroidectomized is similar to that found after total hypophysectomy¹⁰, the posterior pituitary alone cannot maintain sebum secretion, and probably acts by facilitating or augmenting the sebotrophic effects of the anterior pituitary target gland hormones.

Although we have not yet examined the effect of neurohypophyseal hormones on sebum secretion we have shown that α -melanocyte-stimulating hormone (α -MSH), produced in the pars intermedia, stimulates sebum secretion in both intact⁸ and hypophysectomized rats (Thody and Shuster, to be published). Furthermore, α -MSH enhances the actions of testosterone and progesterone on the preputial glands¹². Although we have not yet tested the various forms of β -MSH, β -lipotrophin, a hormone with a similar heptapeptide sequence,

Table 1 Effect of Total and Posterior Hypophysectomy on Body and Organ Weights in the Rat (mean $\pm$ s.e.m.)

Treatment	Body weight (g)		Adrenals	Thyroid	Organ weights (mg)		Prostate
	Initial	Final			Testes	Seminal vesicles	
Intact controls	232 $\pm$ 6	298 $\pm$ 6	49.3 $\pm$ 2.0	15.6 $\pm$ 1.0	3,157.4 $\pm$ 101.5	441.0 $\pm$ 42.0	277.4 $\pm$ 18.3
Total hypophysectomy	238 $\pm$ 7	188 $\pm$ 6	13.1 $\pm$ 1.0 <i>P</i> < 0.001	6.9 $\pm$ 0.5 <i>P</i> < 0.001	514.3 $\pm$ 25.0 <i>P</i> < 0.001	39.1 $\pm$ 4.2 <i>P</i> < 0.001	13.3 $\pm$ 1.9 <i>P</i> < 0.001
Posterior hypophysectomy	260 $\pm$ 14	267 $\pm$ 8	40.3 $\pm$ 5.8 NS	11.0 $\pm$ 1.0 <i>P</i> < 0.01	3,137.0 $\pm$ 143.8 NS	459.3 $\pm$ 41.3 NS	288.7 $\pm$ 36.5 NS

Student's *t* test was used to test the significant difference between operated and control rats.

NS = not significant at 0.05 level of probability.

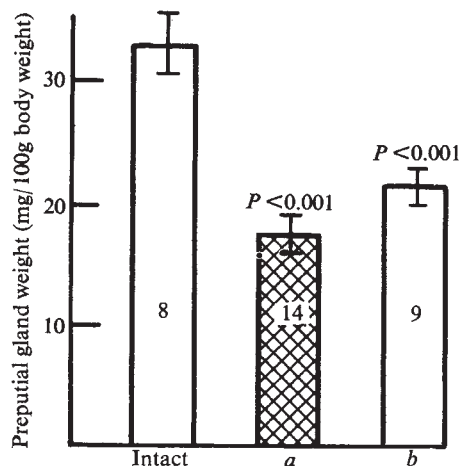


Fig. 2 Effect of total (a) and posterior (b) hypophysectomy on preputial gland weight in the rat (mean $\pm$ s.e.m.). The number of animals in each group are given in the appropriate block. The P values refer to the significant difference when compared with control values.

does contain sebrotrophic activity¹³. We have found an unidentified sebrotrophic substance in pituitary residues¹⁴, and it remains to be shown whether this hormone is related to MSH.

MSH is under inhibitory control and if this hormone plays a part in sebum secretion then sebum secretion might also be controlled by an inhibitory mechanism. Our studies in man and rat suggest that this is the case. Patients with Parkinsonism have an increased sebum secretion and this can be restored to normal by administration of L-dopa¹⁵. This is not due to an inhibitory effect of L-dopa on sebum secretion because L-dopa does not decrease sebum secretion in normal rats (Thody and Shuster, to be published) or normal subjects¹⁶. It therefore seems likely that the lesion of Parkinsonism removes a normally inhibitory influence on sebum secretion which is restored by restoration of brain dopamine.

Patients with breast cancer secrete sebum at an unusually high rate^{17,18}, and this cancer is improved by hypophysectomy. This is assumed to be an effect of the anterior pituitary but could be due to removal of the intermediate zone. The endocrine control of sebaceous glands in breast cancer obviously merits further attention, but in the meantime a clinical trial of the MSH inhibitory factor (MIF) or selective ablation of the intermediate zone would be worthwhile. Adolescent acne is also associated with an increased rate of sebum secretion and its severity is related to the magnitude of the seborrhoea^{19,20}. Attempts to control acne by decreasing sebum secretion have generally involved the inhibition of androgen secretion or activity, methods which have not proved practicable. Our observations provide a new approach to the control of this disorder.

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A. J. THODY
S. SHUSTER

Department of Dermatology,
Wellcome Laboratories for Research into
Skin Disease,
University of Newcastle upon Tyne

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Stereopsis generated with Julesz Patterns in Spite of Rivalry imposed by Colour Filters

It is well known that binocular fusion of slightly disparate images from the two retinæ can produce a sensation of depth. The presence of contours was considered necessary for this effect until Julesz¹ used computer generated "random dot stereograms" to show that a depth percept can occur in the absence of contours, and indeed stereopsis can create contours. The binocular fusion of colours (red and green), on the other hand, has been a highly controversial subject since the time of Helmholtz and Hering. Hecht² produced a yellow fusion with red and green wratten filters but Hurvich and Jameson³ have criticized his interpretation of the fusion.

In attempting to produce more effective "fusion" we tried using coloured outline drawing stereo pairs presented in an old-fashioned stereoscope. A vivid depth effect could be produced even when the drawing in one eye was red and in the other eye was green. The colours, however, showed no tendency to fuse into yellow. Indeed there was colour rivalry and the stereopsis was generated in spite of the rivalry.

Sometimes, however, the two coloured drawings were seen simultaneously; that is the outlines were "double colour"—a red line beside a green line. These results show that depth perception can occur simultaneously with either colour rivalry or diplopia.

In another experiment, to completely rule out any diplopia and to eliminate contours, coloured Julesz patterns were obtained and used instead of outline drawings. For this purpose film negatives were obtained from a printed Julesz pattern pair (from Duke-Elder's *System of Ophthalmology*⁴). The negatives were now placed on a white background and viewed haploscopically. A red wratten filter (No. 29) was interposed between the left eye and left pattern and a green filter (No. 58) in front of the right eye. The central square which stands boldly out in depth is alternately red and green. In no instance are the two colours seen simultaneously (either fused or in diplopia). There is no enhanced fusion of the two colours to produce yellow.

This suggests that concepts of retinal rivalry need drastic revision. During rivalry one retinal image is "shut off" completely from conscious perception. But from our results it is clear that some information from the suppressed image is preserved and utilized. Rather than "retinal" or "eye" rivalry, therefore, there seems to be information rivalry in the brain. In the case of our Julesz pattern experiment, there is "clash" of colour information along the colour transmission lines, leading to rivalry. But the transmission lines for vectorial information are different and, because there is no "clash" of

vectorial information, a sensation of depth is generated simultaneously with rivalry of colour.

In this connexion we thought it would be interesting to find out whether rivalry of form would be compatible with stereopsis. For this purpose the photo negative of a haploscopic Julesz pattern pair was superimposed over a pattern on a card consisting of diagonal lines slanting in opposite directions (one direction for each eye). Such patterns of diagonal lines usually gave rise to rivalry.

Even in these conditions the central square did float out of its background and was seen "in depth". The lines, however, no longer showed rivalry but actually ran criss-cross behind the central square as well as the background. This result shows that fusion of form to produce the stereoscopic depth effect induces a "fusion tendency" in the surround. It could also be argued that it was the Julesz patterns *per se*, and not the stereopsis produced by them, that induced the simultaneous perception of the slanting lines but this does not seem likely to us.

We conclude therefore that though stereopsis is compatible with colour rivalry it tends to induce the fusion or simultaneous perception of even dissimilar forms in the surround.

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V. S. RAMACHANDRAN
S. SRIRAM

Stanley Medical College,
Madras

Received December 30, 1971.

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Evidence for Stabilizing and Apostatic Selection by Wild Blackbirds

WHEN predators concentrate on rare varieties of a prey species, as previous work has suggested¹⁻⁴, there is a tendency towards the elimination of variability. This type of selection is stabilizing⁵. When predators choose the most common varieties, the selection is apostatic⁶ and may result in the maintenance of variability⁴⁻⁹. My results (ref. 10 and in preparation) with wild blackbirds (*Turdus merula* L.) and artificial prey suggest that selection may be stabilizing or apostatic according to the density of the prey.

The prey were cylindrical (0.7 cm long × 0.7 cm diameter) lard-and-flour baits coloured green or brown⁷. In a first set of experiments, during April and May 1968, closely packed baits were distributed at random in two circular (19 cm diameter) metal sieves. One sieve contained 30 greens and 270 browns, the other 270 greens and 30 browns. They were placed 1 m apart on the lawn at the Department of Zoology, University of Edinburgh. Predation was confined to a pair of blackbirds and the number of baits taken was deduced from a frequent examination of the sieves. Eaten baits were replaced to ensure that the 9 : 1 ratios were kept constant. The populations were presented daily for 2 to 4 h and their relative positions were frequently altered throughout the 37 days of the study.

Table 1 gives the grand totals of baits taken and the results expected on the basis of no selection. The blackbirds clearly tended to select the rarer colours. This result is statistically highly significant.

In a second set of experiments, carried out between 1966 and 1969, populations were presented at densities of two baits per square metre on lawns and grass fields near Edinburgh. Including a preliminary study⁷, eleven experiments gave data for blackbirds alone, and in one case predation by a single

Table 1 Grand Totals of Baits taken from 9 : 1 Populations presented at Maximum Density

Dates of presentation (1968)	Population 1 9 green : 1 brown		Population 2 1 green : 9 brown	
	G	B	G	B
April 22—May 27	1,606 (1,978.2)	592 (219.8)	743 (168.5)	942 (1,516.5)

Population size = 300 baits. Figures in parentheses represent the expectation if the baits were taken in proportion to their frequencies. G, Green; B, brown.

songthrush (*Turdus philomelos* Hart.) is included. The populations usually consisted of 200 baits distributed at random in grids of 100 m squares, each metre square containing two baits. In one experiment 100 baits were randomly distributed in a grid of 50 m squares. The populations of three experiments were observed continuously. Others were sometimes left unattended for short periods, after which they were examined to determine the number of baits removed. In each experiment the spatial distributions of the baits were altered daily and eaten baits were replaced frequently. In five experiments the populations were first presented with greens nine times as common as browns, followed by populations with browns nine times commoner than greens. In the remaining six experiments the populations were presented in the reverse sequence. The experiments varied in length, but most lasted for about 2 weeks.

The overall results are given in Table 2. We must enquire whether selection was related to the frequencies of the colours. If selection had remained constant throughout a given experiment the cross-product ratio:

$$\frac{\text{No. of green offered} \times \text{No. of brown eaten}}{\text{No. of brown offered} \times \text{No. of green eaten}}$$

should be the same during both parts of the experiment. The expected grand totals based on this assumption are given in Table 2. In nine cases the deviations from expected were

Table 2 Grand Totals of Baits taken from 9 : 1 Populations presented at Densities of 2/m²

Ex- peri- ment	Nos. of blackbirds	9 green : 1 brown		1 green : 9 brown		$\chi^2_{(n)}$	P
		G	B	G	B		
1*	6+	354 (348.9)	86 (91.1)	1 (3.7)	80 (77.3)	2.38	NS
5.2	4	353 (343.0)	56 (66.0)	11 (17.4)	278 (271.6)	4.32	<0.01
5.3	2	231 (217.0)	40 (54.0)	7 (14.5)	299 (291.5)	8.60	<0.01
5.4	2	192 (175.6)	39 (55.4)	6 (13.6)	348 (340.4)	10.82	<0.01
6.2	4	619 (601.3)	148 (165.7)	5 (13.6)	313 (304.4)	8.13	<0.01
2.1*	2 and 1 songthrush	1 (1.1)	312 (311.9)	92 (91.5)	327 (327.5)	0.01	NS
4*†	2†	0 (3.3)	232 (228.7)	117 (103.1)	74 (87.9)	7.46	<0.05
3.3‡	10†	2 (4.0)	734 (732.0)	63 (50.5)	102 (114.5)	5.44	<0.02
5.1	4	3 (13.5)	368 (357.5)	372 (348.0)	90 (114.0)	15.18	<0.001
5.5	5	3 (14.2)	350 (338.8)	380 (356.1)	81 (104.9)	10.25	<0.001
6.2	4	4 (16.5)	606 (593.6)	411 (375.4)	131 (166.6)	20.72	<0.001

Population size = 200 baits except where shown. Figures in parentheses represent the expectation assuming constant selection. G, green; B, brown; NS, not significant.

* Continuously observed.

† The two "9 : 1" populations were separated by a period of presentation of "1 : 1" populations.

‡ Population size = 100 baits.

statistically significant, but such direct comparisons of observed and expected numbers are not strictly legitimate, because of heterogeneity within the experiments^{7,10}. In all eleven experiments the blackbirds took a larger number of common forms than expected and a smaller number of rare ones. This proportion of "successes" is statistically significant ($P < 0.001$, binomial test). The results therefore appear to be consistent, despite heterogeneity.

To summarize, selection by wild blackbirds seems to be stabilizing when baits are at a very high density and apostatic when the density is lower. Predation would presumably be random at some intermediate density. At very high prey densities predators may tend to form "specific searching images"¹¹ for uncommon varieties, because they stand out against a background of the common sorts^{2,4} or because they are odd^{1,3}. When prey are at low densities predators will have to search more actively and may profit by the formation of specific searching images for common varieties⁶. Predation may be random at very low prey densities¹¹. What constitutes "very high", "low", or "very low" densities of prey in a given environment will depend on the particular species of predator and prey.

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JOHN A. ALLEN

Department of Zoology, PO Box 35064,
University of Dar es Salaam,
Dar es Salaam, Tanzania

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Sexual Selections by Variations in Fitness at Breeding Time

ACCORDING to the original theory of sexual selection, Darwin¹ thought that some males would gain a selective advantage over others if they had characteristics that improved their chances of mating. The theory required polygamy: the stronger males who drove away the weaker would collect more females to mate with. The females might also have a mating preference for a particular male characteristic. Thus the elaborate mating displays of males in some species might have evolved. The female mating preference would continue to evolve at the same time^{2,3}. General models of this process have shown that such sexual selection could produce rapid evolution of both male characters and female preferences⁴. Female mating preferences are well known in *Drosophila*^{5,6}.

Darwin⁷ later described a model of sexual selection in monogamous species. In his own words, he described the model as follows. "Let us take any species, a bird for instance, and divide the females inhabiting a district into two equal bodies, the one consisting of the more vigorous and better nourished individuals, and the other of the less vigorous and healthy. The former, there can be little doubt, would be ready to breed in the spring before the others. There can also be no doubt that the most vigorous, best nourished and earliest

breeders would on an average succeed in rearing the largest number of fine offspring. The males, as we have seen, are generally ready to breed before the females; the strongest, and with some species the best armed of the males, drive away the weaker; and the former would then unite with the more vigorous and better nourished females, because they are the first to breed. Such vigorous pairs would surely rear a larger number of offspring than the retarded females, which would be compelled to unite with the conquered and less powerful males, supposing the sexes to be numerically equal; and this is all that is wanted to add, in the course of successive generations, to the size, strength and courage of the males, or to improve their weapons." According to this model therefore there must be a correlation between breeding time and fitness: unless the earlier pairs produce more offspring the superior males will not get a selective advantage. Fisher⁸ gave an interesting numerical example of Darwin's theory. Lack⁹ criticized the theory on the grounds that breeding time had evolved in response to the time when food was available for the young and the female could form eggs, and said that "Darwin's and Fisher's suggestions are not very cogent". But the factors Lack mentions are the physiological and ecological causes of variations in fitness in relation to breeding time; Darwin's theory, however, depends on the selective consequences of these variations in fitness among males who mate with females breeding at different times: thus Lack's criticism is irrelevant. In this article, data on breeding time and fitness are used in a computer model to show how selection will proceed according to Darwin's theory. A model of a more general theory shows that selection can still take place even when the earliest females are not the fittest. The selection is always frequency-dependent.

To construct the computer model, data are required of the variations in fitness of pairs of mates breeding at different times. I¹⁰ analysed such data as an example of selection for a quantitative character, which was the time of breeding of a large number of pairs of the Arctic skua, a sea bird of the North Atlantic. Breeding time was measured by the date when the first egg hatched. The variations in fitness admirably fitted a quadratic "fitness function" of the relation between the fitness, measured by the proportion of chicks reared, and the breeding time, measured in weekly intervals, by the hatching of the first chick¹¹. The quadratic fitness function takes the form $w = 1 - A - K(\theta - x)^2$ where w is the fitness, x the breeding time in weeks and A and K are constants. Thus at $x = \theta$, $w = 1 - A$ which is the point of maximum fitness. Hence θ is the optimum breeding time. If A is equal to zero, then the fitnesses are relative and $w = 1 - (\theta - x)^2/\phi$ where $\phi = 1/K$. From the data, we get estimates of $\theta = 0.47354$ and $\phi = 24.254$ and these values give the fitnesses and frequencies shown in Table 1.

The results of selection depend on the proportion of males who gain an advantage by forming pairs in the earlier breeding periods. Suppose a dominant gene determines a male character which gives sexual superiority to its possessors; this may be a character like strength or a particular threat display that enables the male who has it to drive away those who do not. Or it may be a character for which there is a female mating preference. In Darwin's theory, the sexually superior males all gain mates before any of the others do so. More usually, however, they may not be able to gain so complete a sexual

Table 1 Fitness of Pairs of Arctic Skuas breeding at Different Times

Breeding period, x (in weeks from time of first breeding)	No. of pairs	Fitness, w , according to the equation $w = 1 - (\theta - x)^2/\phi$ (ref. 11)
0	20	0.99075
1	114	0.98857
2	61	0.90393
3	23	0.73683
4	3	0.48726

advantage. Only some of the females may exercise a mating preference while the rest mate at random regardless of whether the males possess the sexually advantageous character or not. Since mating preferences can be selected for and must therefore be at least partly genetically determined^{12,13}, the proportion of females having the gene that determines the preference must also increase as a result of the selection⁴. This is because the mating preference gene will be passed on through the females' sons who will, on average, have the superior male phenotype like their fathers before them.

Table 2 shows the results of computer simulations of selection when a dominant gene determines the sexual superiority of the males. α is the proportion of females who actually exercise a mating preference for the superior males. The selective coefficient given in the table measures the relative disadvantage of the inferior males as the selection proceeds. (The details of these and other more extensive calculations will be published elsewhere.) Table 2 shows an interesting fact about this kind of selection: it is frequency-dependent and the frequency-dependence is positive or negative depending on α . Thus when α is small, the selection is negatively frequency-dependent: as the superior males increase in frequency the selective coefficient at first increases slightly but then starts to decline and becomes quite small at the finish. The superior males are then at only a slight selective advantage. As α increases and a higher proportion of the superior males gets the sexual advantage, so the negative frequency-dependence changes to positive. When $\alpha = 1.0$ the selective coefficient increases rapidly with frequency and ends at a very high value.

When the earliest pairs have the highest fitness, as shown in Table 1, there must always be a selective advantage for males who mate first: the earliest pairs necessarily have a fitness higher than the mean fitness of the whole population. But unless the higher fitness of the earlier pairs is entirely environmental, as Darwin supposed, and caused by the chance nutritional conditions of the females, the mean breeding time will itself evolve towards an optimum. Suppose that the breeding times are normally distributed with the optimum breeding time at the mean: according to the fitness function $w = 1 - (\theta - x)^2/\varphi = 1 - (\bar{x} - x)^2/\varphi$, the fitnesses will decrease symmetrically from the mean towards earlier and later breeding. The first pairs to breed then have as great a disadvantage as the last.

Suppose there is a complete ($\alpha = 1.0$) female preference and all the sexually superior males mate first. If they are at a frequency of less than 50%, they will form pairs whose average fitness is less than that of the rest, for they are all breeding before the optimum. Thus the preferred males will be at a

Table 3 Initial and Final Selective Coefficients for Given Values of α and Given Initial Gene Frequencies when Pairs breeding at the Mean are the Fittest

Values of α	Initial gene frequencies				
	0.1	0.2	0.3	0.5	
0.1 S_0	0.00700	0.00521	0.00375	0.00255	
S_∞	0.00194	0.00194	0.00194	0.00194	
0.2 S_0	0.00382	0.00933	0.00869	0.00641	
S_∞	0.00482	0.00482	0.00482	0.00482	
0.3 S_0	-0.00326	0.00955	0.0124	0.0118	
S_∞	-0.501	0.00914	0.00914	0.00914	
0.4 S_0	-0.0109	0.00730	0.0141	0.0177	
S_∞	-0.501	0.0156	0.0156	0.0156	
0.5 S_0	-0.0183	0.00376	0.0139	0.0232	
S_∞	-0.501	0.0254	0.0254	0.0254	
0.6 S_0		-0.00371	0.0125	0.0277	
S_∞		-0.501	0.0405	0.0405	
0.7 S_0		-0.00470	0.0103	0.0308	
S_∞		-0.501	0.0651	0.0651	
0.8 S_0			0.00756	0.0329	
S_∞			0.107	0.107	
0.9 S_0			0.00451	0.0341	
S_∞			0.183	0.183	
1.0 S_0			0.00121	0.0346	
S_∞			0.334	0.334	

selective disadvantage. If the preferred males are at a frequency of 50%, they will form pairs breeding up to the mean breeding time and have exactly the same fitness as the others because the fitnesses are symmetrical round the mean. They will have no selective advantage or disadvantage. Above 50% they will have the advantage. If the preference is not complete ($\alpha < 1.0$) and only some of the sexually superior males are chosen by the earliest females, others will be left, perhaps to be chosen later. If a sufficient number of these males are left by the earliest females and mate later to form pairs breeding chiefly at about the mean breeding time, they may gain an overall advantage whatever their initial frequency. Table 3 shows a calculation of the selective coefficients in the selection of a dominant gene when the optimum breeding time is at the mean. (The breeding times were given a variance equal to that of the Arctic skua.) Two selective coefficients are shown. One is the initial selective coefficient, s_0 , when selection starts with a given frequency. The other is the final selective coefficient, s_∞ , when the gene is on the point of being fixed in the population or eliminated. When the gene frequency is high ($p = 0.5$), the males necessarily get an advantage. But when $p = 0.1$ they have an advantage only when α is small (less than 0.3). When p is very small

Table 2 Gene Frequency and Selective Coefficient during the Sexual Selection of a Dominant Male Character starting at a Gene Frequency of 0.001

No. of generations	$\alpha = 0.2$		$\alpha = 0.4$		$\alpha = 0.6$		$\alpha = 0.8$		$\alpha = 1.0$	
	Gene frequency	Selective coefficient	Gene frequency	Selective coefficient	Gene frequency	Selective coefficient	Gene frequency	Selective coefficient	Gene frequency	Selective coefficient
1	0.00103	0.0590	0.00103	0.0590	0.00103	0.0590	0.00103	0.0590	0.00103	0.0590
50	0.00467	0.0594	0.00467	0.0594	0.00467	0.0594	0.00467	0.0594	0.00467	0.0594
100	0.0216	0.0603	0.0218	0.0612	0.0218	0.0615	0.0218	0.0615	0.0218	0.0615
120	0.0394	0.0621	0.0400	0.0626	0.0403	0.0631	0.0403	0.0637	0.0403	0.0638
140	0.0716	0.0646	0.0731	0.0666	0.0738	0.0669	0.0742	0.0673	0.0742	0.0676
160	0.118	0.0539	0.133	0.0755	0.135	0.0759	0.136	0.0762	0.136	0.0764
180	0.171	0.0442	0.222	0.0691	0.243	0.0894	0.247	0.0989	0.248	0.0994
200	0.223	0.0367	0.322	0.0631	0.380	0.0949	0.418	0.119	0.444	0.151
220	0.271	0.0311	0.411	0.0563	0.512	0.0907	0.594	0.148	0.658	0.254
240	0.314	0.0276	0.484	0.0505	0.611	0.0895	0.714	0.159	0.804	0.308
260	0.353	0.0251	0.544	0.0469	0.682	0.0848	0.791	0.167	0.880	0.455
300	0.422	0.0220	0.633	0.0430	0.771	0.0807	0.868	0.163	0.942	0.478
350	0.492	0.0198	0.709	0.0407	0.833	0.0788	0.911	0.162	0.965	0.478
400	0.549	0.0184	0.761	0.0396	0.869	0.0779	0.933	0.161	0.975	0.478
600	0.696	0.0162	0.864	0.0381	0.932	0.0770	0.967	0.161	0.989	0.477
1,000	0.824	0.0152	0.930	0.0376	0.966	0.0768	0.984	0.161	0.995	0.477

The fitnesses of the pairs of mates are shown in Table 1, the earlier pairs being the fitter.

the value of α must also be very small for an advantage to be gained from the female preference.

As Table 3 shows, when the gene frequency is high enough for the selective coefficient to be positive, the selection becomes positively frequency-dependent at the higher values of α . The frequency-dependence is therefore the same as when the earlier females are the fitter. In general, however, it is unlikely that fitness would be symmetrically distributed around the mean breeding time. Many species of birds lay more eggs and fledge more chicks if they breed early. The earlier pairs are then fitter and selection always favours the sexually superior males.

Darwin was probably right when he suggested that earlier breeders are fitter. But sexual selection does not depend on this fact. It will certainly be much quicker and always effective if the earlier breeders are more fit; but even if they are not, an advantage can still be gained if the sexually superior males are sufficiently numerous or if sufficiently few of the females exercise a mating preference. The mating preference will necessarily appear at first among only a few females. The selection of the preferred males will therefore take place, but it will be slow since α is small. As the mating preference itself evolves and α increases, the frequency of the preferred males will also increase and they will retain their selective advantage. The sexual selection will therefore continue to increase the frequency of both the males and the females who exercise a preference for them. It will become positively frequency-dependent with the preferred males finishing at a considerable selective advantage.

The calculations described in this communication were carried out using the Titan Computer at the University of Cambridge.

PETER O'DONALD

Department of Genetics,
University of Cambridge,
Milton Road, Cambridge CB4 1XH

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Cranial Morphology of Neanderthal Man

RECENT attempts^{1,2} to explain the features of Neanderthal remains in terms of rickets and treponemal disease assume that such features cannot be of adaptive significance or reflect geographical variation. They further imply that the differences between the European Neanderthals and the succeeding Upper Palaeolithic populations are too great to be accounted for in terms of phyletic change and polytypism. Consequently, the European Neanderthals are considered an isolated or aberrant population whose distinctive morphology results from genetic drift or pathological changes. Mayr and Campbell³ have criticized these assumptions and have drawn attention to the marked diversity between approximately contemporary but geographically separate populations during the late Pleistocene.

My work indicates that the European Neanderthals are cranially more similar to Upper Palaeolithic *Homo sapiens* than is generally appreciated, and that attempts to categorize the early Würm Neanderthals as pathologically deformed are ill founded, at least as far as the features of the skull are concerned.

I have recently completed an analysis of evolutionary change in hominids using multivariate techniques to describe the total morphological pattern of the skull. The hominid groups included samples of European Neanderthals (Neanderthal, Spy I and II, La Chapelle-aux-Saints, La Ferrassie I, La Quina I, Gibraltar I, Mt Circeo, Ochos), Middle East Neanderthals (Tabun I and II, Skhul IV and V) and European Upper Palaeolithic *Homo sapiens* (Cromagnon I and III, Combe-Capelle, Engis, Grotte des Enfants II, Brno I and II, Predmost III and IV) as well as Lower and Middle Pleistocene hominids and a sample of modern individuals ($n=17$). All data on fossil hominids were from measurements on casts.

Altogether, 105 characters were devised to describe the hominid skull, which was divided into eight functional complexes as follows (number of characters in each complex in parentheses): (a) upper face—supra-orbital torus, orbits, nasal bones and nasal aperture (16); (b) upper jaw—maxilla, palate (11); (c) mandible (16); (d) cheek region and masticatory musculature—zygomatic processes of maxilla and temporal, zygoma, extent of masseter and temporalis muscles (12); (e) articular region—size and proportions of articular eminence, glenoid fossa and glenoid tubercle (8); (f) balance—length of anterior and posterior cranial segments, size and orientation of occipital condyles and extent of nuchal area (14); (g) cranial vault—size and proportions of frontal, parietals, squamous temporals and occipital bones (16); (h) basicranium—size and proportions of foramen magnum, basioccipital, basisphenoid and petrous temporals (12). Sixteen characters were also selected from each of the above complexes to provide a measure of overall skull proportions.

Discriminant function (canonical variate) analysis was carried out on the hominid data for each complex, and generalized distances (D^2) between the groups were obtained using Gower's Q -mode analysis⁴ rather than the more usual R -technique. The statistical distance (ref. 5)—the square root of D^2 —between each pair of Würm populations for each complex is given in Table 1, together with the significance levels of the D^2 values. The purpose of the analysis was to obtain a measure of morphological distance for each complex between hominid groups which have been previously determined on chronological and geographical criteria rather than

Table 1 D Distances between Late Pleistocene Human Groups for Different Cranial Complexes

Complex	European Neanderthals—Middle East Neanderthals	European Neanderthals—Upper Palaeolithic <i>H. s. sapiens</i>	Middle East Neanderthals—Upper Palaeolithic <i>H. s. sapiens</i>
(1) Upper face	8.5†	5.1†	7.6†
(2) Upper jaw	2.4	1.9	3.4
(3) Mandible	3.0	5.4†	5.6†
(4) Cheek region and masticatory musculature	3.5	3.5*	7.7†
(5) Articular region	1.2	0.8	0.9
(6) Balance	1.9	4.3†	4.4†
(7) Cranial vault	8.5†	8.9†	5.4†
(8) Basicranium	4.8*	4.5†	1.3
(9) Overall	5.2†	8.4†	8.7†

* Indicates D^2 value significant at 1% level.

† Indicates D^2 value significant at 0.1% level.

discrimination as such, when D^2 is used to assign a doubtful specimen to a particular group to facilitate classification.

The figures indicate that some cranial complexes (upper face, mandible, balance, cranial vault) have changed considerably during the Upper Pleistocene, whereas others (articular region, upper jaw) show little change over the same period, thereby emphasizing the markedly mosaic nature of morphological change in hominid evolution. This is reflected by the varying significance of the D^2 values obtained: those based on the analyses of articular and maxillary regions do not differ significantly and could well result from sampling populations homogeneous for the characters chosen; those derived from the upper face, balance and neurocranial complexes indicate marked diversity between early-middle Würm populations in these regions ($P < 0.001$).

The D values also reveal that although both Neanderthal groups differ significantly from the Upper Palaeolithic sample in most complexes, the "classic" Neanderthals are closer to the succeeding European specimens in all facial and gnathic complexes, and that only in the neurocranial complexes do the Middle East Neanderthals show greater similarity to the Upper Palaeolithic group. In overall skull morphology, as measured by the combination of characters selected from each complex, the European Neanderthals are closer—although not appreciably so—to the Upper Palaeolithic sample than is the Mount Carmel material.

Particularly interesting is the relative proximity of the European Neanderthal and Upper Palaeolithic samples in the upper face and masticatory complexes, because the "classic" group is often considered to show distinctive morphological characteristics in those regions. In the cheek region and masticatory musculature the distance between the Mount Carmel and Upper Palaeolithic samples (7.7 units) is more than twice that between the latter group and the "classic" Neanderthals (3.5 units), corresponding to a D^2 value more than four times as large and a significance level of $P < 0.001$, whereas that between the two European groups is $0.001 < P < 0.01$.

The D distance between the "classic" Neanderthal and Upper Palaeolithic samples derived from the analysis of upper face dimensions is similarly much less than that between the Middle East specimens and the later material. Both D^2 values are highly significant ($P < 0.001$), indicating rapid evolutionary change in facial parameters during early and middle Würm. The value obtained between the Mount Carmel and Upper Palaeolithic groups, however, is more than twice that separating the European material, making it most improbable that the differences are attributable solely to limited sampling, and indicating a facial morphology for the Middle East population quite distinct from that of the two European groups. This is confirmed by the marked separation (8.5 units) of both Neanderthal groups on the basis of this complex.

The characters used in the multivariate analysis were designed to measure such features as the proportions of the supra-orbital torus, the size and angulation of the nasal bones, the size and shape of the orbits and nasal aperture (upper face) and the extent of origin of the masseter, the length of the zygomatic arch and its relation to the body of the maxilla (masticatory apparatus), all of which have been used to differentiate between the European Neanderthals and *H. s. sapiens*^{6,7}. The analyses may thus be expected to maximize the separation of the "classic" Neanderthals and the Upper Palaeolithic sample. On the other hand, the Mount Carmel specimens lack most of the features noted above and, although prognathous, are considered to possess otherwise modern facial proportions^{7,8}.

The fact that the total morphological pattern of the European Neanderthals in the face and cheek regions is closer to the succeeding Upper Palaeolithic sample than that of the Middle East Neanderthals indicates that the distinctiveness of these features has been exaggerated and that emphasis on individual characters in isolation results, as in the case of the Swanscombe

remains⁹, in a distorted view of morphological affinity. Certainly there is no reason to consider that any of the facial characteristics of the European Neanderthals result from pathological changes. On the other hand, if some or all of these features are adaptive⁷, it is not surprising that the European Neanderthals, living in the same geographical region as the Upper Palaeolithic individuals under approximately equivalent climatic conditions, should resemble them facially more closely than do the Middle East Neanderthals.

The multivariate analysis does indicate that the total morphological pattern of the cranial vault (long neurocranium, low but broad frontal, laterally expanded biparietal arch, angulated occipital and "bunning") and basicranium (elongated foramen magnum, long basioccipital, long sphenoid body, reduced basicranial flexion) of the European Neanderthals is distinctive. This is indicated by the wide separation of the classic sample from both other groups and by the highly significant D^2 values obtained from these complexes. Their cranial proportions, however, hardly correspond to those characteristic of the suggested pathological conditions, as Mayr and Campbell note³. A much more convincing explanation is that the differences reflect polytypic variability, corresponding to localized phyletic development from late *H. erectus* or early (Holstein or Eem) *H. sapiens* populations. Evidence supporting this view is provided by the fact that the European Neanderthal sample is close—8.8 D units—to early *H. sapiens* (Steinheim and Swanscombe) in the overall morphology of the cranial vault, and by the fact that the two Neanderthal groups show fewer significant D^2 values between one another than either does from Upper Palaeolithic *H. s. sapiens*.

Because hominid evolution during the late Pleistocene was probably reticulate in pattern¹⁰, and there are short time intervals (<25,000 years) between geographically diverse samples, it is not possible to distinguish precisely between the phyletic and polytypic components of morphological change. Probably both sources of variability are reflected in the transition from Neanderthal to *H. s. sapiens* in Europe, because the archaeological evidence suggests movement of human groups into Europe at the end of the Paudorf interstadial and the beginning of late Würm. The available data indicate that there is no reason to consider that the European Neanderthal crania are pathologically deformed; early and late Würm European populations seem to be cranially much more similar than is often thought.

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A. BILSBOROUGH

Department of Physical Anthropology,
Faculty of Archaeology and Anthropology,
University of Cambridge,
Downing Street,
Cambridge CB2 3DZ

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BOOK REVIEWS

Optimistic Jeremiah

The Closing Circle: Confronting the Environmental Crisis. By Barry Commoner. Pp. 336. (Jonathan Cape: London, January 1972.) £2.50.

IN his earlier book *Science and Survival*, published just ten years ago, Barry Commoner appeared as one of the earliest and most forceful of the scientific jeremiahs, pointing to the potentially disastrous consequences of thoughtless applications of modern technology. That book was quite largely concerned with the hazards to mankind of the designing and testing of nuclear weapons, and it played a worthy part in the movement which has been successful in achieving a considerable degree of control over the dangers of radioactive fall-out, and a hazardous stalemate in the deployment of atomic weapons themselves. In his new book Commoner is not concerned with the dangers of "the atom", and confines himself to the interrelated problems of population growth and environmental pollution. The arguments are still forcefully presented, and are of a type unlikely to appeal to the tribe of Dr Pangloss, but the book makes an impression of considerably greater maturity than the earlier one, and one is not unduly surprised to discover at the end of it that Commoner declares himself an optimist.

Commoner begins by arguing that the dangers to the natural environment are much more obvious and imminent in the developed than in the developing countries and do not arise primarily from pressure of population or a rise in affluence, but rather from changes in technology most of which have been introduced since the end of World War II. During this period the population of the United States increased by a little over 40 per cent, and the output of basic goods has done little more than keep pace with this, except in the field of personal transportation. There has been a considerable increase in the vehicle-miles travelled *per capita* per year—much of it due to the shift of urban populations into the suburbs, much further away from their places of work. Commoner provides figures to

show that the increase in pollution of various kinds has been much greater, often by at least an order of magnitude. For example, the total number of vehicles on the United States roads increased 166 per cent between 1947 and 1968, but "studies of the amounts of lead deposited yearly in glaciers show that the annual entry of lead into the environment, almost entirely from gasoline, has increased by about 400 per cent in the last 25 years", while "a reasonable estimate of the increase in smog levels in United States cities since World War II would be ten-fold or so".

In most connexions, the increase in pollution has been much faster than in production. Commoner argues that this is a consequence of a number of changes that have taken place in production technology, both in industry and in agriculture. One tendency has been to substitute artificial materials for natural ones; for instance, synthetic fibres and plastics for cotton, wool and cellulose products. The synthetic materials often involve types of chemical bonds which do not occur in the natural environment and which cannot be degraded by the normal decomposing organisms. Moreover, drastic chemical means may be needed to synthesize these bonds, which leads to such results as an increase since 1946 of 2,100 per cent in the use of mercury for chlorine production in the United States. There has also been a tendency to move from materials such as steel and timber, which require rather little energy for their production, to materials such as aluminium which requires very much more. Then new systems of agricultural production, such as reliance on heavy applications of fertilizers and pesticides, and on intensive management of livestock, increase the polluting effects of production much more than its overall efficiency.

Commoner's conclusion is that "the increased output of pollutants per unit of production, resulting from the introduction of new productive technologies since 1946, accounts for about 95 per cent of the total output of pollutants, except in the case of passenger travel, where it accounts for about 40 per cent of the total".

Commoner goes on to examine the reasons for these changes in production and technology, and argues that they are a consequence of the fact that neither in capitalist nor in communist countries does the economic system at present take account of the full social costs of the goods which it produces. An adequate cost-benefit analysis should include the impact of the production processes on the natural environment; but the costs of that impact are borne by society at large, and not by the customer for the particular goods produced, so it gets left out of the equation which determines the profitability or assessed efficiency of the production process. Moreover, when profit is an explicitly recognized goal of the exercise, as in capitalist countries, there is an inevitable tendency to cash in on the high profits which are characteristically earned in the first few years after a new product has been discovered and produced.

Commoner pays little attention to the political implications of his analysis of the causes of the pollution of the environment. He finds that the basic fault is an inadequate *philosophical* outlook; "this fault is reductionism, the view that effective understanding of a complex system can be achieved by investigating the properties of its isolated parts". It is this outlook, he believes, that has led to the one-sided emphasis on the single factor of immediate profit or productive efficiency, and the neglect of "the inevitable side-effects that arise because, in nature, no part is isolated from the whole ecological fabric".

Commoner accepts that it would be exceedingly difficult to design and operate an economic system which does adequately allow for all these side-effects, but he does not find the task impossible. He argues that "the world is now no longer willing to tolerate even the present level of environmental degradation, much less its intensification". However, "ecological survival does not mean the abandonment of technology. Rather it requires that technology be derived from a scientific analysis that is appropriate to

the natural world on which technology intrudes". . . "Since the environmental crisis is the result of the social mismanagement of the world's resources, then it can be resolved, and man can survive in a humane condition when the social organization of man is brought into harmony with the ecosphere".

Commoner's book is primarily concerned with the situation in the highly industrialized countries. He does, indeed, devote a chapter to the problems in developing countries, in particular their population. He points out that there are almost certainly tendencies towards the regulation of human populations, acting by a reduction of birth rate when a high enough standard of living has reduced both the overall death rate and in particular the infant mortality rates. He seems to be more optimistic than many authors in feeling that these factors can come into operation early enough to prevent there being major problems of over-population in most of the developing countries. He is probably too single-minded in refusing to allow the population problem to divert him from his main thesis—the valuable partial truth that several major recent technologies do greater harm to the environment than is warranted by their increased efficiency. It is a good point—but far—very far—from being the whole of the problem we face in trying to work our way into the twenty-first century.

C. H. WADDINGTON

Feral into Farm Species

The Origin of the Domestic Animals of Africa. By H. Epstein. Revised in collaboration with I. L. Mason. Vol. 1. Pp. xi + 573. Vol. 2. Pp. xi + 712. (Africana Publishing: New York, London and Munich, December 1971.) \$85; £35.50 two volumes.

IN the log-book of Vasco da Gama's first voyage in 1497 the cattle of the Hottentots at what is now called Mossel Bay are described as "very big like the Alemejo cattle, very fat, reddish of colour and very wild". A hundred and fifty years later Jan van Herwaerdens found "numerous cattle which were much bigger than those in the homeland where we had never seen cattle of such size". Many of these large animals had a prominent cervico-thoracic hump, and their prevailing colour was red, although others were known. The Dutch East India Company began breeding cattle in 1673 to provision their numerous ships which revictualled at the Cape; war had broken out between the Dutch and their cattle suppliers, the Cochoqua

Hottentots, who had sold the Cape Peninsula to the Company the previous year. The herds taken from the defeated Cochoqua, who never regained them, became the sole or principal ancestors of the famed Africander breed of draft oxen. These cattle formed the beautiful spans of the transport riders before the days of railways, and made possible the Great Trek.

In 1927 Professor Epstein took up stock farming in the western Transvaal and became interested in the history of the Africander breed which, after two centuries as trek-oxen, was being improved for beef production. At that time very little was known on the subject and he found it difficult to gather the few scattered facts. "It soon became apparent that to trace the path of the ancestral stock of the Africander cattle and of the Hottentots, their former breeders, it was necessary also to pay attention to the other domestic animals of the Hottentots. And since the origin of the animals of the Hottentots could not be fully understood without a comparison of their racial characteristics with those of the domestic breeds of the Bantu, the theme gradually expanded until it finally embraced the various types and breeds of domestic animals throughout the whole of Africa".

More than forty years of research have gone to the making of this encyclopaedic work, which is based on the author's direct experience of breeding and handling livestock in Africa, Asia, Europe and America. To this he has added years of close study of agriculture and archaeology, animal husbandry and zoology, natural and human history and prehistory. The result is a book that will be the standard for many years to come, and brings such a vast mass of scattered information together that everyone interested in the subject will make it the starting point for any further researches.

Volume 1 deals with the dogs, cattle and buffalo of Africa, and volume 2 with the sheep, goat, pig, ass, horse and camel. Each chapter opens with a survey of the wild species from which the domestic breeds may have descended, and this is followed by detailed descriptions of the morphological, physiological, taxonomic and distributional characters of the African breeds and varieties. The author then traces back the historical and prehistorical record to the place of origin of the various animals in Asia, Europe or Africa. He attempts, by this close attention to descent, to establish not only the relationships of the domestic breeds to the wild species and to extinct domestic forms, but also to each other and to the domestic breeds of other continents.

Professor Epstein presents an enor-

mous gathering of facts and theories in words and pictures, which show that the attempt to unravel the threads woven into the tapestry depicting the origin of the domestic breeds of Africa is indeed a task to try the patience of a Penelope. Some broad conclusions can be drawn, but such is the complexity of the matter that any detailed understanding seems to be beyond achievement. Man has been stirring the gene pool of his domestic animals for five or six millenia, not understanding what he did, and now it seems to be too late to know exactly what happened during the brew.

These fascinating volumes are copiously illustrated, have an extensive bibliography, and three indexes. Professor Epstein pays generous tribute to his able collaborator and critic, I. L. Mason. It is not surprising that no ordinary publisher could be found to bring out this book, for the returns from publishing a book on this scale without wide popular appeal are meagre. In spite of subsidies from the South African Council for Scientific and Industrial Research, and from the South African Friends of the Hebrew University of Jerusalem, its price puts it beyond the reach of all but the most enthusiastic workers on the subject, and even librarians will hesitate before adding it to their shelves. This is a pity because it deserves to be widely available.

L. HARRISON MATTHEWS

History of Aether

Nineteenth-Century Aether Theories. By Kenneth F. Schaffner. (The Commonwealth and International Library of Science, Technology, Engineering and Liberal Studies: Selected Readings in Physics.) Pp. ix + 278. (Pergamon: Oxford and New York, January 1972.) £3.25.

THIS book contains reprints from the writings on theories of the aether by a number of nineteenth century physicists, including texts drawn from the works of Fresnel, Stokes, Green, MacCullagh, Kelvin, Fitzgerald, Larmor and Lorentz. Inevitably one can quarrel with the selection—the omission of Maxwell seems odd—but Professor Schaffner has chosen an interesting selection of sources. The book includes a historical essay on theories of the aether in nineteenth century physics, and the references to this commentary serve as a guide to further primary and some recent secondary sources.

Any historical work on this subject is inevitably indebted to E. T. Whittaker's *History of the Theories of Aether and Electricity: The Classical Theories* (1951), and Schaffner fully

acknowledges his debt. Schaffner's historical essay is straightforward, and he tends to follow Whittaker too much in his overall conception of the period. Thus he argues that aether theories were at first attempts to explain optical phenomena mechanically, and that later attempts were made by men such as Larmor and Fitzgerald to incorporate Maxwell's electromagnetic theory of light within the framework of a mechanical theory of the aether. Though he recognizes that Larmor's theory has to be seen as an attempt to develop a theory of reality in terms of an electromagnetic aether, Schaffner fails to demonstrate the way theories of this kind relate to some of the most problematical features of Maxwell's work. As Maxwell recognized, his electromagnetic theory of light threatened the very presuppositions of a mechanical theory of the aether. Some of the tensions in Maxwell's own ideas appear in his referee's report on Fitzgerald's paper (written in 1879, and to which Schaffner refers) where he writes of the ambiguities involved in the relation of mechanical to electromagnetic theories. Moreover, Schaffner tends to adopt uncritically some of Whittaker's anachronisms (as in his account of Maxwell); this is not only unhistorical, but tends to blur some of the finer conceptual points.

Nevertheless, Whittaker's judgments were often quirky, and Schaffner does not follow him in his curious assessment of Einstein's special theory of relativity. More important to this book is Schaffner's discussion of MacCullagh, for, as he quite rightly points out, MacCullagh's theory of the aether was widely held to be dynamically unsound (as Stokes emphasized in 1862), and Whittaker's statement that this theory was in accordance with the laws of dynamics is incorrect.

In spite of its almost inevitable weakness, this is a competent account of an under-explored and highly complex set of problems. Schaffner's crisp summaries are lucid and provide a useful introduction to this subject.

P. M. HEIMANN

Parasites

Animal Parasites. By J. G. Baer. Translated from the French by Kathleen Lyons. Pp. 256. (Weidenfeld and Nicolson: London, January 1972.) £2.10 hardback; £1.05 paperback.

ACCORDING to the publishers, the aim of the series to which this volume belongs is "in an age of increasing specialization . . . to provide authoritative introductory books for students which will be of interest also to the general reader". In a biological sub-

ject, in which advances follow one another with bewildering rapidity, this is clearly a difficult and a challenging task. Professor Baer has used his immense knowledge of parasitology to produce a volume which goes a long way towards fulfilling these aims. Not only does he deal with the parasitic protozoa, helminths and arthropods—but the (much-neglected) parasitic molluscs also receive considerable attention. Some of the examples he selects are unusual and were certainly new to me. The more theoretical aspects of the subject—adaptation, host-parasite relationships and evolution—are also discussed, although other parasitologists may not always agree with some of Professor Baer's views in these controversial areas. The volume is well produced and illustrated, but more complete labelling of the text figures would have added much to their value from the point of view of the student. Apart from the more physiological and biochemical aspects of parasitology, on which the author only touches, the book is a useful general introduction to the subject both for the layman and the student. The task of the translator is never easy and Dr Lyons is also to be congratulated on producing a most readable text.

J. D. SMYTH

Drug Dangers

Drugs of Abuse: Their Genetic and other Chronic Non-psychiatric Hazards. Edited by Samuel S. Epstein. Pp. xiv + 228. (MIT: Cambridge, Massachusetts, and London, December 1971.) \$15; £7.

THE papers in this symposium volume are those of a conference held at San Francisco in October 1969, which like two earlier gatherings was prompted by considerable public alarm over possible genetic implications in the use of LSD. In a way these apprehensions have, like so many recent scares, been misdirected; to quote from Lederburg and Epstein's preface:

"The possible biological hazards of such drugs like LSD which derive from conjecture as much as from controlled experiments, have attracted interest disproportionate to more plausible concern on direct and residual psychotropic effects."

In fact the scope of the book is considerably wider. It deals not only with the non-therapeutic use of drugs as practised in the "drug cultures" but also with the mutagenic and carcinogenic hazards which the ever increasing products of the chemical industries add to our food, drink and to the environment in general. There are useful contributions dealing with the chemistry, sources, epidemiology and non-behavioural pharmacology of the

various drugs of abuse; on carcinogenic, teratogenic and genetical hazards. Further methodological essays describe mutagenic testing with microbes and *Drosophila*, cytological tests and statistical estimation of mutation rates in man. Three appendices deal with the psychiatric perspectives on drug abuse, the US National Institute of Mental Health and its Division and Study Center of Narcotic Addiction and Drug Abuses. The first provides, so to say, the centre piece, to which the rest of the book can be regarded as a kind of marginalia; it deals concisely with the core of the problem, namely the subjective experiences for which drugs are taken by the individual and the psychiatric aberrations and behavioural consequences of drug taking, which constitute the problem for society. The last two appendices give organizational information.

The contributions to the volume are all readable and apparently meant to be authoritative summaries for the non-specialist, that is, fellow scientists in other related fields. There is a great deal of factual information, tabulated or otherwise, useful bibliographies at the end of each paper and a general index.

H. KALMUS

Crystal Theory

Color and Symmetry. By Arthur L. Loeb. Pp. xiii + 179. (Wiley: New York and London, November 1971.) £5.

CLASSICAL crystallography is a deductive science which has been established for more than 70 years. Its main results have been proved in many different ways, both geometrical and algebraic. Yet fresh formulations of the theory are still being developed, no doubt motivated by its ever-increasing importance in crystal physics. Loeb generates two-dimensional infinite nets by the continuing interaction of non-equivalent rotocenters. Only five non-trivial possibilities are admissible which conform to his "postulate of closest approach". Coupling the rotocenters systematically to mirror and glide planes produces seventeen distinct symmetry configurations, which prove to be essentially identical to the seventeen classical plane groups. The scheme is then expanded by the introduction of colour reversal rotocenters, thereby confirming the existence of the Shubnikov colour groups (forty-six black-and-white plus the seventeen disputed grey plus the seventeen monochromatic). Finally, and this seems to be original, symmetry patterns are introduced which involve up to twelve different colours. There appear to be

eighty-six distinct plane polychromatic colour groups, each one carefully illustrated by a coloured diagram. Loeb's analysis enables him to make illuminating comments on the Shubnikov "grey" groups, and also on the celebrated periodic colour patterns created by M. C. Escher.

It cannot be said that this book makes easy reading. In the first place Loeb does not clearly declare his premises and objectives, nor does he relate his point of view to the axioms of the classical theory. Certainly his generated monochromatic and dichromatic symmetries coincide with those previously discovered, but I can see no immediate mathematical reason why this should be so. In the second place his approach is entirely geometrical, resulting in a diffuse "chain of theorems" which provides little insight into the mutual implication of operators. Most interested students will probably translate this argumentation into algebraic terms in order to convince themselves that they really understand it. My final conclusion is that Loeb has produced an important original work, which may not make the impact it deserves owing to an inadequate presentation. M. A. JASWON

Education

Higher Education: an International Journal of Higher Education and Educational Planning. Editorial Board: A. M. Ross, E. A. van Trotsenburg, H. J. Butcher, R. O. Berdahl, Noel J. Entwistle and G. L. Williams. Pp. 140. (Elsevier: Amsterdam, February 1972.) One volume of four issues per year: Personal subscriptions: Dfl. 42.00, £5.10. Subscriptions for institutes: Dfl. 77.00, £9.35.

THE growth of higher education in the past twenty years has made the market for books and magazines about it a large and growing one. In response to this market, a whole series of books and magazines have been supplied, of which the latest is *An International Journal of Higher Education and Educational Planning*, called *Higher Education*, of which the first number is now to be reviewed.

It is a thin and expensive production, and the first issue consists of four major articles. Two of them are, in effect, repetitions of work by fairly well-known people, Professor Eysenck and Professor Blaug, the one on psychology and the other on economics. Both of their articles are entirely predictable and present no new work. In the one case Professor Eysenck argues that psychological theory should be tested, and inevitably the testing leads to a confirmation of his views on extroverts and

introverts. Professor Blaug is a great believer in the market, and inevitably his work suggests that the market works well if it is made to work well.

The two other articles are more interesting. The first is an attempt to apply a systems approach to education in general, by Erich Jantsch, which covers a certain amount of territory that has been fairly well traversed in the past, but which systematizes this range of literature in a useful and interesting way. The last article, by Professor Carter, on the efficiency of universities, is a useful contribution to an analysis of the meaning of a whole series of controversial words. It covers much of the field that I have myself covered in other work. Naturally, I find it a good article because it confirms my own findings.

JOHN VAIZEY

Computing in Chemistry

Computing Methods in Quantum Organic Chemistry. By H. H. Greenwood. Pp. xi+213. (Wiley: New York and London, February 1972.) £5.50.

THE Hückel molecular-orbital method for dealing with planar conjugated and aromatic molecules was introduced in 1931; and it represented for many years the most successful and simple illustration of the use of wave mechanics in organic chemistry. At the present time several thousand papers are published each year using some form of this method to discuss some molecule or some property of a molecule of interest to organic chemists. There have been many books describing the basic theory; but none so far that goes into detail about the numerical and computational techniques to be used. Dr Greenwood's book remedies this defect.

The book is solely concerned with π electrons. It concentrates most of its effort on the simple Hückel theory; but after pointing out some of the limitations of this method, it devotes the last quarter of the pages to a consideration of the Roothaan equations which go beyond Hückel theory. This includes a few of the many simplifications associated with the names of Pariser, Parr and Pople (and others) which neglect most of the tiresome many-centre integrals and, by suitable parametrization, give good results both for ground and excited states.

Simple derivations are given for atomic charges, bond orders, free valences, reactivity indices, polarizabilities; and in each case these are immediately followed by the outline of a suitable program, written in Fortran II, which will evaluate the quantities numerically. There is also a discussion of dipole moments, and also of π - π

bonding. However, there is no account of any theory, such as those based on the extended Hückel or CNDO approximations, which attempts to include both σ and π electrons at the same time. The explanations are simple and straightforward: taken all in all, especially for those without access to the Quantum Chemistry Program Exchange facilities, this is a useful book.

C. A. COULSON

Pteridophytes

Pollen and Spore Morphology/Plant Taxonomy: Pteridophyta. By G. Erdtman and P. Sorsa. Pp. 302. (Almqvist and Wicksell: Stockholm, 1971.) n.p.

IN spite of the title this volume is described in the authors' preface as presenting morphological data without taxonomic speculation; in nearly 200 pages all pteridophyte genera are listed alphabetically with data on selected species in brief note form. It is intended to be used as a completion of the pteridophyte part of Volume II (1957), and the list of genera has been updated (Copeland system). Cross-referencing to Volume II is thorough, but there seem to be slight lapses in assistance to the reader as in the case of *Hemitelia*. Reference is made to the many other works in which the appropriate spore descriptions may be found, but in this volume there are no line-drawing illustrations as in Volume II. A set of twelve plates (sixty figures) of scan photographs is included but they are not particularly impressive in most cases and the magnifications used appear to be random; the selection policy of illustrations in relation to those in Volume II is not clear. There is a useful table of genera (pp. 191-8), showing nature of aperture, presence of perine, and maximum size. There is a warning in the preface that not all determinations have been checked; this is, of course, a difficult but fundamental problem in such work if taxonomy is eventually to be involved.

Two other papers are included in the volume. John M. Pettitt on "Some ultrastructural aspects of sporoderm formation in pteridophytes" deals very briefly with heterosporous pteridophytes, providing seventeen excellent plates of electron micrographs. Barbro Gullvåg on "The fine structure of some pteridophyte spores before and during germination" presents twenty-three excellent electron micrographs with about ten pages of breathless scattered wisdom; it is interesting but editorially badly arranged. It is not clear why it should be included in this book rather than as a contribution to a regular journal in which normal referee comment would surely have improved the presentation.

N. F. HUGHES

CORRESPONDENCE

Aharon Katzir Katchalsky

SIR,—We the undersigned were scientific colleagues and friends of Aharon Katzir Katchalsky. Your readers will know of his tragic death, in company with 25 others, at the hands of terrorists in the airport at Lod. We write not only to mourn the loss of an old friend and one of the greatest scientists to emerge from Israel, but also to express our horror at so indiscriminate and so futile a waste of rare human resources.

Aharon Katchalsky was vigorous in the pursuit of scientific research of a most distinguished kind, and also found time to play a leading part in all kinds of international organizations and activities to the extent that he seemed to be a citizen of the world rather than

of any one country. In both respects his loss is irreparable, and there is especial irony in the fact that this indiscriminate act of slaughter destroyed a man of deep humanity who was not only an eminent scientist but also happened to be particularly involved in furthering the cause of peace in the Middle East. It would not be proper or useful for us to propose remedies for a situation where such events can take place, but we would express our opinion that a continuation of the escalating spiral of retaliation and counter-retaliation is unlikely to be effective, but will merely multiply the number of innocent victims.

For all these reasons, we now pro-

pose that there should be established a Katchalsky Memorial Fellowship tenable at the Weizmann Institute. The ideal would be that scientists from all countries should be enabled to spend periods of up to a year in Israel on research or study in some way connected with the varied work for which Katchalsky was distinguished. We urge that our colleagues elsewhere should send expressions of their support for this scheme to the Editor of *Nature*, who—we are told—is discussing practical arrangements with the Weizmann Institute.

Yours faithfully,

F. H. C. CRICK,
*MRC Laboratory of Molecular Biology,
University Postgraduate Medical School,
Hills Road, Cambridge.*

J. C. KENDREW,
*MRC Laboratory of Molecular Biology,
University Postgraduate Medical School,
Hills Road, Cambridge.*

ALAN HODGKIN,
*Physiological Laboratory,
Cambridge.*

R. D. KEYNES,
*ARC Institute of Animal Physiology,
Babraham, Cambridge.*

M. F. PERUTZ,
*MRC Laboratory of Molecular Biology,
University Postgraduate Medical School,
Hills Road, Cambridge.*

M. WILKINS,
*Department of Biophysics,
King's College, London.*

PETER F. CURRAN,
Yale University School of Medicine.

ISADORE EDELMAN,
*University of California School of
Medicine, San Francisco.*

FRANKLIN HUTCHINSON,
*Department of Biophysics,
Yale University.*

WALTER A. ROSENBLITH,
Massachusetts Institute of Technology.

A. A. SOLOMAN,
*Biophysical Laboratory,
Harvard Medical School.*

CORNELIUS TOBIAS,
*Department of Medical Physics,
University of California, Berkeley.*

J. WATSON,
Cold Spring Harbor Laboratory.

ROBERT MONIER,
*CBM-CNRS,
Marseilles, France.*

F. BUCHTHAL,
*Institute of Neurophysiology,
Copenhagen, Denmark.*

O. MAALØE,
*University Institute of Microbiology,
Copenhagen, Denmark.*

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*Max-Planck-Institut für Zellchemie,
Munich, Germany.*

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Stockholm, Sweden.*

H. THEORELL,
Sweden.

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*Basel Institute for Immunology,
Basel, Switzerland.*

G. M. FRANK,
*Institute of Biophysics,
Moscow, USSR.*

LEV KAYUSHIN,
*Institute of Biophysics,
Moscow, USSR.*

Obituary

Professor G. van Iterson

PROFESSOR GERRIT VAN ITERSON, Dutch botanist, technologist, and founder of applied research organizations in the Netherlands, died in Wassenaar on January 4, 1972, aged 93. For over 40 years, from September 1907 until August 1948, Professor van Iterson held the chair of Technical Botany in the Delft Institute of Technology, the same institute where, in 1901, he had majored in chemical technology and subsequently had been

invited by the famous microbiologist M. W. Beijerinck to become his assistant.

Working with Beijerinck, van Iterson acquired a considerable knowledge of biology and published a number of papers on denitrifying bacteria and on the aerobic decomposition of cellulose by microorganisms. In 1907 he presented his thesis *Mathematische und mikroskopisch-anatomische Studien über Blattstellungen, nebst Betrachtungen über den Schalenbau der Miliolinen* which far exceeded in size,

plan and contents everything normally offered as a thesis, and was the most convincing proof of the high scientific talents of the author. Beijerinck encouraged him to become a general botanist and, feeling the need for instruction to future technologists about industrial plants and their products, he asked the government to create a chair in microscopical anatomy at the Delft Technical Institute. Needless to say, van Iterson became the first occupant of this chair.

Van Iterson's new duties left him

little enough time to continue theoretical work on phyllotaxis; work he only resumed after his retirement.

But as a teacher, although his subject was specialized, van Iterson attracted many students to his lectures which went far beyond the official specifications of his chair. His practical courses were also models of instruction. In addition his laboratory was an active centre of research on a great variety of problems, usually with relation to industrial plants or their products, but not infrequently of a purely botanical nature—for example, his work on the extension growth of young cells. About 160 publications by van Iterson and his students and colleagues emerged from his laboratory including 22 doctoral theses.

Van Iterson was not content, however, with creating his own subject and teaching it. He soon realized that if the knowledge he disseminated was to bear fruit in the long run, it needed close ties with industries and development companies. His ultimate aim was to convince industrialists of the value of scientific research.

The rubber industry was the first to receive his attention. In 1910, on van Iterson's instigation, the government decided to establish an advisory service for the rubber trade and industry. The service was started with the appointment of one of van Iterson's assistants and housed in a room in his small laboratory. After several years of extensive activity a similar advisory service was started in 1919 for the textile and papermaking industries. He encouraged research on wood, and did a difficult task in reorganizing in the Netherlands East Indies the experimental station for the so-called upland cultures.

Finally, in 1924, van Iterson also became a member of a small committee to advise the government on

how to ensure that scientific research benefited the public. The recommendations of the committee resulted in the Netherlands Organization for Applied Scientific Research (TNO). Van Iterson became a member, and subsequently president, of the executive committee. Today this organization employs some 4,000 people in industrial or governmental research, and its many subdivisions include institutes for rubber, fibres and wood—continuations of van Iterson's earlier initiatives.

It is remarkable that van Iterson's membership of numerous committees in industrial and public organizations did not affect the meticulousness with which he performed his university duties. In addition he was a member of many learned societies, serving on the board of a number of them, including the Royal Netherlands Academy of Sciences, the Biological Council and the Chemical Council of the Netherlands. He was a honorary doctor of the University of British Columbia and he earned two royal distinctions.

There are several lines leading from van Iterson to England. In particular Dr Ir. A. Wijnberg, his first graduate, who demonstrated the possibility of recovering on a commercial scale the hard wax present in the waste press cakes from cane sugar factories, and subsequently became one of the founders of the beet sugar industry in Britain. Further, van Iterson, though 20 years his junior, was a good friend of Sir d'Arcy Wentworth Thompson who sometimes visited him at his home in Delft. Finally, van Iterson, realizing the possibilities of X-ray diffraction for the resolution of fine structure in natural products, sought contacts with X-ray crystallographers in England working in biological fields, in particular W. T. Astbury and R. D.

Preston of the University of Leeds. As a result Delft physicists were encouraged to develop X-ray facilities for use by investigators in different fields, and van Iterson initiated research which gave new insights into the wax coatings of plants, the molecular organization of starch grains, and the cell walls of lower plants.

Van Iterson was careful and straightforward in conversation, and a good listener with a critical but warm sense of humour and unbounded energy. After retiring in 1948 van Iterson continued his personal research until, fifteen years later, declining health forced him to stop. His death is a great loss to all those who gained from his gifts as a leader, a scientist and an organizer.

Announcements

University News

Dr Robert L. Wildey, US Geological Survey Center of Astrogeology, has been appointed associate professor of astrophysics and astronomy at **Northern Arizona University**.

Dr I. D. Cooke, Welsh National School of Medicine, has been appointed to the chair of obstetrics and gynaecology in the **University of Sheffield** and **Mr P. H. Burke**, United Cambridge Teaching Hospitals, has been appointed professor of dental health.

Appointments

Mr D. J. Barron, chairman of Rowntree Mackintosh Limited, has been appointed to membership of the **University Grants Committee**.

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